Did Legalized Abortion Lower Crime?

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ABSTRACT

This paper examines the relationship between the legalization of abortion and subsequent decreases in crime. In a current study, researchers estimate that the legalization of abortion explains over half of the recent decline in national crime rates. The association is identified by correlating changes in crime with changes in the abortion ratio weighted by the proportion of the criminal population exposed to legalized abortion. In this paper, I use an alternative identification strategy. I analyze changes in homicide and arrest rates among teens and young adults born before and after 1970 in states that legalized abortion prior to *Roe v. Wade*. I compare these changes with variation in homicide and arrest rates among cohorts from the same period but who were unexposed to legalized abortion. I find little evidence to support the claim that legalized abortion caused the reduction in crime. I conclude that the association between abortion and crime is not causal, but most likely the result of confounding from unmeasured period effects such as changes in crack cocaine use and its spillover effects.

I. Introduction

In a recent and controversial article, Donohue and Levitt (2001) present evidence that the legalization of abortion in 1973 explains over half of the recent decline in crime across the U.S. Moreover, they suggested that the full impact on crime of *Roe v. Wade* will not be felt for another 20 years. To quote, “Our results suggest that all else equal, legalized abortion will account for persistent declines of 1 percent a year in crime over the next two decades” (p. 29). If true, policies that reduce abortion would be expected to increase crime whereas policies that increase abortion, such as the recently approved abortion pill, might have the opposite effect. Given the social costs associated with crime and the controversy surrounding abortion, a causal link between abortion and crime has profound implications for social policy.

The purpose of this paper is to analyze the association between abortion and crime. Donohue and Levitt present a compelling case that the association between abortion and crime is potentially causal. According to the authors, increases in the ratio of reported abortions to live births, the abortion ratio, are strongly associated with lower crime and arrest rates. Moreover, the magnitude of the effect is remarkable. A 50 percent increase in the mean abortion ratio is associated with an 11 percent decrease in violent crime, an 8 percent decrease in property crime and a 12 percent decrease in murder. These effects are generally larger and more precisely estimated than the effects of incarceration and police manpower. Thus, the magnitude of the association between abortion and crime, the use of non-experimental methods, and the controversial nature of the policy implications suggest that further examination is warranted.

My empirical approach to the analysis of abortion and crime differs from that of Donohue and Levitt and follows previous efforts to associate legalized abortion with fertility, schooling, and child poverty (Levine et al. 1999; Angrist and Evans 1999; Gruber, Levine and Staiger 1999). Specifically, I estimate a reduced form equation in which changes in arrest and homicide rates among cohorts exposed to legalized abortion in the period prior to *Roe* are compared with cohorts unexposed.[1] In five states, Alaska, California, Hawaii, New York and Washington and the District of Columbia—henceforth the repeal states—abortion was *de jure* or *de facto* legal for at least two years prior to *Roe*.[2] I also use national legalization following *Roe* as a second experiment. A link between abortion and crime should reduce crime among cohorts from non-repeal states born after *Roe* relative to similar groups in repeal states.

There are several advantages to a reduced-form analysis of abortion and crime that uses categorical measures of abortion legalization rather than reported legal abortions. First, abortion was poorly measured in the early years of legalization. Surveillance systems for recording abortion were operating in only 14 states by December of 1970 (Centers for Disease Control 1971). The Alan Guttmacher Institute (AGI) did not collect data on abortion until 1973. Donohue and Levitt (2001) use only AGI data and thus have no data on abortion prior to 1973, although earlier data from the CDC are available.[3] They also use abortions by state of occurrence and not state of residence. Thus, New York and New Jersey, for example, had abortion ratios (abortions per 1000 live births) as measured by state of occurrence of 913 and 113, respectively, in 1973.[4] When abortions are measured by state of residence, the ratios for New York and New Jersey are, respectively, 553 and 431 (Forrest, Sullivan and Tietze, 1979).

Second, not only is abortion poorly measured in the early 1970's, but even when abortion

[1] A recent manuscript by Lott and Whitley (2001) also focuses on a comparison of cohorts exposed and unexposed to legalized abortion. They report a *positive* but relatively small association between legalized abortion and murder rates.

[2] Washington D.C. has not been treated as an "early legalizer" in previous analyses. However, the 1969 decision in *United States v. Vuitch* rendered the District's abortion law unconstitutional. As a result, writes Lader, "Washington's abortion facilities soon ranked among the busiest in the country, with 20,000 patients in 1971" (Lader, p. 115).

[3] For the five states that legalized abortion prior to *Roe v. Wade*, Donohue and Levitt simply backcast from 1973 totals. For the other 45 states and the District of Columbia they assume the abortion ratio was zero.

[4] Abortion ratios by state of occurrence are all abortions performed in a state, regardless of where the woman resides, divided by the number of births to residents of that state. By contrast, abortion ratios by state of residence are all abortions to residents of that state, regardless of where they are performed, divided by births to residents of that state.

reporting improves, it may still be a poor proxy for unintended births averted. Donohue and Levitt assume that differences in abortion ratios within states over time reflect differences in unintended childbearing. Few would contend that abortion is an unambiguous measure of unwanted pregnancy. But it does not follow that differences in the growth of abortion within states in the period after *Roe* measure relative changes in pregnancies that would have become unintended births. For instance, between 1970 and 1975 the U.S. fertility rate fell from 87.5 to 66.0 births per 1000 women 15 to 44; the reported legal abortion rate over this same period rose from 5 abortions per 1000 women 15 to 44 in 1970 to 18 in 1975. However, from 1975 to 1980 the fertility rate *rose* to 68.2 despite a 39 percent increase in the abortion rate from 18 to 25 (Koonin et al. 1999).[5] Such summary statistics are at best suggestive. Nevertheless, they underscore the possibility that the link between abortion and unintended fertility is weaker than Donohue and Levitt assume.

A third advantage of the reduced form with categorical measures of abortion legalization is that previous studies have demonstrated a marked decline in relative fertility rates in repeal states between 1971 and 1973 (Berkov and Sklar 1974; Joyce and Mocan 1990; Levine et al. 1999; Angrist and Evans 1999). This decline in fertility rates represents a more credible source of identifying variation with which to uncover a relationship between abortion and crime than within-state changes in reported legal abortions between 1973 and 1985.

Finally, a reduced form analysis of arrests and homicides by single year of age allows for a simple but direct comparison of cohorts exposed and unexposed to legalized abortion. Did, for example, the arrest rate for murder among 18-year-olds fall, or increase less, in repeal relative to non-repeal states among those born between 1969 and 1971?

[5] The number of abortions are based on the Centers for Disease Control and Prevention (CDC) survey of state health departments, which tend to be lower than totals reported by the Alan Guttmacher Institute (AGI). I use the CDC estimates because AGI did not report abortions for 1970.

I conclude that the association between abortion and crime is not causal, but most likely the result of confounding between the growth in abortion and changes in crack cocaine and handgun use. My conclusion is based on the following findings.

First, the abortion rate in repeal states between 1975 to 1985 is almost double that of non-repeal states. Yet the fertility rate in both groups of states is essentially identical. If greater abortion is not associated with lower fertility, especially among teens, then it is unclear why states with greater abortion rates should have lower rates of unintended childbearing.

Second, an analysis of arrest and homicide rates by single year of age indicates that teens born between 1968-1973 in repeal states who come of age between 1986 and 1991 experience similar or greater increases in crime than teens in non-repeal states, a finding counter to the Donohue/ Levitt hypothesis. However, the same experiment for young adults 20 to 24 years of age often reveals large relative decreases in crime among those born in repeal as compared with non-repeal states. Different results by age group explain in part Donohue and Levitt's findings, since their estimates are based primarily on the association between abortion and crime in the 1990's. Yet the discrepancy by age group is also inconsistent with a strong cohort effect and suggests that confounding from unmeasured period effects, such as the crack epidemic, are likely important.

Furthermore, I find no association between legalized abortion and crime in analyses that use within-state and within-race comparison groups to control for period effects. Thus, the dramatic decline in homicide rates among blacks 20 to 24 years of age before and after exposure to legalized abortion is no different from the decline that occurred among black teens who experienced no change in exposure to legalized abortion over the same period.

Third, when I use changes in fertility rates from 1968 to 1973 between repeal and non-repeal states to identify relative changes in unintended childbearing, there is no significant association between fertility and crime. These regressions are persuasive for several reasons: 1) the well-documented decline in fertility associated with legalized abortion in the pre-*Roe* period appears clearly related to the decline in unintended childbearing; 2) the accuracy with which births as compared to abortions are measured in the 1960's and early 1970's eliminates measurement error; and 3) the availability of fertility rates by race increases the power to detect an effect given the greater demographic impact of legalized abortion on black fertility.

Finally, Donohue and Levitt's primary regressions are sensitive to functional form. It is not clear *a priori* what the correct functional relationship between crime and unintended childbearing should be. Donohue and Levitt regress the natural logarithm of crime on the level of the effective abortion ratio. When I run the exact same regressions but use the log of the effective abortion ratio, the coefficient on abortion becomes positive and statistically significant, the complete opposite of what they report.

II. Empirical Issues

There are two empirical challenges to establishing a link between abortion and crime. First, one must demonstrate a relationship between abortion and unintended childbearing. Clearly, aborted pregnancies are unwanted. The proper counterfactual, however, is unwanted or mistimed pregnancies that would have become unintended births had abortion rates been lower. New York, for example, has a greater abortion rate than Texas. But New York may not have a lower rate of unintended childbearing if the difference in abortion represents substitution from abstinence and contraception.

The second challenge is to isolate a cohort effect, such as legalized abortion, from strong age and period effects. There is growing consensus that the crack/cocaine epidemic engendered a wave of violence that began in the mid-1980's and peaked in the early 1990's (Blumstein 1995;

Cork 1999; Blumstein and Wollman 2000). Moreover, the upsurge in crime was concentrated almost exclusively among those under 25 years of age, and affected blacks more than whites. Unless one controls for these sources of confounding, it is difficult to convincingly attribute the decline in crime to the legalization of abortion.

A. Abortion and unintended childbearing

As outlined by Donohue and Levitt, there are several ways in which legal abortion can affect crime. Cohort size is one. Fewer births mean fewer criminals in subsequent years. Legal abortion, however, may also affect crime rates by allowing women—predominantly poor, young, and minority—to avoid an unintended birth. Since children from disadvantaged backgrounds are more likely to commit crimes as teen or adults, the result of this selective reduction in unintended childbearing is a drop in crime rates approximately 15 to 25 years later.

There are two components of unintended childbearing through which abortion may operate. Legal abortion may improve the timing of births. As a result, children are born into more nurturing environments than they would have been had access to abortion been more restrictive. Legal abortion may also reduce family size, which may increase resources devoted to each child. Evidence consistent with a reduction in completed family size would be an inverse relationship between abortion and fertility. States with greater abortion rates would have lower fertility rates, all else constant. Evidence that abortion improves the timing of births would be more difficult to document. After an initial decline in fertility rates with the advent of legalized abortion, there may be little association between abortion and fertility rates as delayed childbearing is completed. However, even if abortion only improves the timing of births, there still should be an inverse association between abortion and teen fertility. That is, states with greater abortion rates should have fewer mistimed births and lower rates of adolescent childbearing, since teens initiate the cycle of delayed fertility.[6]

A third piece of evidence linking abortion and unintended childbearing would be an inverse association between abortion rates and the proportion of births described as unintended. Several surveys ask women that give birth whether in the period just before conception, they had intended to become pregnant or if not, whether the pregnancy was mistimed or not wanted ever. Thus, states with greater abortion rates should have fewer births from pregnancies described as unintended.

The most convincing evidence that legalized abortion had a differential effect on unintended childbearing comes from analyses of fertility rates in repeal states and non-repeal states in the period prior to *Roe*. Early legalization is associated with a decline in relative fertility rates of approximately six percent between 1971 and 1973 (Levine et al. 1999; Angrist and Evans 1999). Given the suddenness with which laws changed in California and New York, the relative decline in fertility would seem a reasonable estimate of unintended births averted. In fact, the relative decline in births may be a better measure of unintended childbearing than the relative increase in abortions. For instance, the rise in legal abortion greatly exceeded the decline in births. Tietze (1973) estimates that two-thirds of the 67,000 legal abortions to New York City residents between July, 1970 and June 1971 replaced illegal abortions. Moreover, legalization of abortion may also have induced substitution from abstinence and other forms of contraception to abortion. Akerlof, Yellen and Katz (1996) show a large jump in the percent of 16-year-old girls with sexual experience around 1970. Indeed, national legalization of abortion following *Roe v. Wade* appears to have had relatively little impact on fertility rates, despite rapid

[6] It is noteworthy that recent work on the impact of delayed fertility among teens has found much less of an effect on schooling and labor market outcomes than previously estimated (Geronimous and Korenman 1992; Grogger and Bronars 1993; Hotz, McElroy and Sanders 1999). More relevantly, a comparison of sisters one of whom gave birth during adolescence and the other

growth in reported legal abortions (Evans and Angrist 1999). The point is that the relationship between abortion and unintended births averted is not obvious given the replacement of legal for illegal abortion in the early 1970's, the increase in sexual activity due to better methods of fertility control, and the potential substitution of abortion for contraception.

Yet demonstrating a relationship between reported abortion and unintended births averted, especially in the period after *Roe,* is essential to the credibility of Donohue and Levitt's identification strategy. Their primary evidence of a link between abortion and crime consists of pooled time-series regressions of the uniform crime rate on what they term the "effective abortion rate." [7] The latter is an average of state abortion ratios from 1970 to 1985 weighted by the proportion of arrestees born since abortion became legal. [8] Thus, the effective abortion ratio in 1996 is the abortion ratio in 1975 weighted by the proportion of 20-year-olds arrested (an additional lag for pregnancy), plus the abortion ratio in 1976 weighted by the proportion of 19-year-olds arrested, continuing in this manner up to the abortion ratio in 1985, weighted by the proportion of arrests of 10-year-olds. By 1997, abortion ratios after 1973 account for 75 percent of the effective abortion rate for property crime, 59 percent for violent crime and 52 percent for homicide.

What I show below, however, is that the abortion rate is uncorrelated with fertility rates in non-repeal states and positively correlated with fertility rates in repeal states in the period after *Roe*. Such results are not inconsistent with Donohue and Levitt's hypothesis since abortion may improve the timing of births without affecting completed fertility. However, even with a timing

who delayed until adulthood found no difference in birth outcomes or cognitive development among the children of the sisters (Geronimous and Korenman 1993; Geronimous, Korenman and Hillemeier 1994).

[7] Donohue and Levitt (2001) refer to the ratio of abortions per 1000 live births as the abortion rate. Demographers refer to this as the abortion ratio. The abortion rate is the ratio of abortions per 1000 women 15 to 44 years of age. I use the terminology of demographers (Henshaw and Van Vort 1992).

[8] Recall that Donohue and Levitt only have data on abortion from 1973 to 1985. In 45 states plus the District of Columbia they assume the abortion ratio was zero between 1970 and 1972. For the other five states they simply backcast from 1973 totals.

story, it must still be the case that abortion is inversely related to teen fertility. If it is not, then the unobserved rate of unintended childbearing may be the same in states despite very different abortion rates. There may be greater sexual activity and more substitution of abortion for contraception in states with ready access to abortion providers, Medicaid finance abortions, and few restrictions such as parental consent statutes or mandatory delay laws. The result may be more pregnancies and more abortions than in states where the cost of abortion is substantially greater (Kane and Staiger 1996). Under this scenario, differences in abortion rates among states, or differences in the growth of abortion within states, would not reflect lower rates of unintended births averted, since the additional pregnancies that resulted in abortion might not have been conceived in a more restrictive environment. This would undermine Donohue and Levitt's strategy of using changes in abortion post-*Roe* to identify unintended births averted.

B. Age and Period Effects

By linking crime to lagged abortion, Donohue and Levitt claim to have uncovered strong cohort effects for an outcome characterized by dramatic age and period effects. Violent crime surged in the mid 1980's, peaked in the early 1990's and began a dramatic decline that has yet to level off. The rise in crime appears related to the arrival of crack cocaine and the increased use of handguns. Nevertheless, when adjusted for such age and period effects, it should still be the case that cohorts exposed to legalized abortion commit fewer crimes than cohorts unexposed.

Donohue and Levitt present time-series evidence that crime grew slower or fell faster between 1988 and 1994 among the states that legalized abortion prior to *Roe v. Wade* as compared with states in which abortion became legal with *Roe*. They also show that exposure to abortion as measured by the effective abortion ratio is highly correlated with arrest rates of persons under 25 and uncorrelated with arrest rates of those 25 and over, important evidence since almost all adults over 25 were born before abortion became legal.

Although such evidence is surely consistent with their hypothesis, potential confounding from strong period and age effects cannot be dismissed. Donohue and Levitt have no data on cocaine use or the spread of handguns. State and year fixed effects may not overcome this deficit. The arrival and spread of crack markets varied significantly by city and state (Golub and Johnson 1997; Cork 1999; Grogger and Willis 2000).[9] Moreover, it is generally agreed that crack markets developed first in poor, urban, predominantly African-American communities. Not coincidentally, the rise and fall in crime was concentrated among the young, primarily black, from central city neighborhoods (Blumstein 1995; 2000; Cook and Laub 1998). Thus, even in models with state and year fixed effects, the relationship between abortion and crime may be confounded by within-state interactions among age, year, and race. A crude solution is to include controls for state-specific linear or quadratic trends. However, this is not possible in the context of their model because the trend terms remove all variation in the abortion ratio.[10] Consequently, Donohue and Levitt have no convincing way to eliminate possible confounding from such state-specific trends.[11]

[9] In a footnote, Donohue and Levitt argue that the "dampening effect" of abortion on crime can be outweighed in the short-run by factors such as crack. But that begs the question of why a similar type of confounding might not occur as crack markets and crime rates declined in the 1990s.

[10] The adjusted R-square in a regression of the effective abortion ratio on state dummies, year dummies and state-specific linear trends is over .99 which explains the sensitivity of Donohue and Levitt's estimates to the inclusion of state-specific linear trend terms (Donohue and Levitt 2001, Table 5).

[11] Donohue and Levitt use "long differences", the change in crime and abortion between 1985 and 1997, as a means of eliminating the entire crack epidemic. However, this reduces the analysis to a bivariate regression of 50 observations of aggregate data. The potential for omitted variable bias would seem rather large.

III. Empirical Analysis

A. Data

Several data sets are used in the analysis. I analyze the crimes related to property, violence and murder from the FBI's Uniform Crime Reports from 1985 to 1997. These are the same data used by Donohue and Levitt (2001), but they are neither age nor race-specific, a serious limitation given my empirical strategy. Thus, I also analyze homicide victimization from the National Center for Health Statistics' Multiple Causes of Death files for the years 1985-1997. Determination of homicide in these files is based on reports by local medical examiners that are compiled nationally by National Center for Health Statistics (NCHS). The two advantages of homicide victimization data are completeness and detail. Not only do total homicide victims as reported by NCHS closely match totals obtained from the Uniform Crime Reports, but they also have information on the victim's age, race, state of residence and state of birth. The primary disadvantage is that victims are not offenders. Although homicide victimization rates have the same time-series pattern as do homicide commission rates, most adolescent victims aged 13 to 17 were killed by someone older, most often someone 18 to 24 years of age (Cook and Laub 1998). Thus, homicide victimization data may be a good proxy for relative changes in crime, but are weaker as a measure of the level of offenses by particular age groups. Nevertheless, these data are widely used by criminologists to analyze patterns in crime because of their completeness and demographic information. (Blumstein 1995; Fagan, Zimring and June Kim 1998; Wintemute 2000).

I also analyze homicide offenses as recorded on the FBI's Supplemental Homicide Reports (SHR) [Fox 2000]. The obvious advantage is that they provide information on perpetrators by age, race and state of residence. The biggest drawback to these data is their reporting deficiencies. Information on the age and race of the offender when missing is imputed based on the known distribution by age/race/sex of victims and offenders by state and year (Maltz 1999). As with victimization data, Supplemental Homicide Reports are widely used to track crime by age and race (Maltz 1999; Cook and Laub 1998; Fox and Zawitz 2000)). Moreover, I use them in conjunction with homicide victimization and murder arrest rates. Thus, a consistent relationship between abortion and crime across the various measures of homicide provides an important check of these data.

In addition, I analyze arrest rates by single year of age for teens and young adults 15 to 24 years old, also from the Uniform Crime Reports. Donohue and Levitt (2001) also analyze arrests by single year of age, which provides an important point of comparison.

Three other data series are important to my analysis. Population data by state and single year of age are from the U.S. Census Bureau; I use them to compute age-specific arrest and homicide rates by state. I also use population by state and race for teens ages 15 to 19 and young adults ages 20 to 24. In addition, I use fertility rates from 1961 to 1985 as published by the National Center for Health Statistics.[12] As I argue above, changes in fertility rates around the time of legalization may be a more accurate measure of unintended births averted than abortions. Moreover, fertility rates by state and race (white, nonwhite) and are available for the oldest cohort in the analysis. Thus, I can associate race-specific crime rates for teens and young adults to appropriately lagged race-specific fertility rates.

Finally, I use information on pregnancy intention to assess whether states with higher abortion rates have fewer unintended births. Pregnancy intention is measured by asking women who give birth whether in the period just before conception they wanted to be pregnant at that time or sooner (an intended pregnancy), whether they wanted to become pregnant later (a mistimed pregnancy), or whether they did not want to be pregnant either now or in the future (an

[12] I thank Phil Levine of Wellesley College for these data (see Levine et al. 1999).

unwanted pregnancy). Data are from Pregnancy Risk and Assessment Monitoring System, or PRAMS, collected by the Centers for Disease Control and Prevention. Initiated in 1988, PRAMS is an annual stratified random sample drawn from birth certificates of approximately 2000 recent mothers per year in each participating state. Three states participated in PRAMS in 1988 (Maine, Oklahoma and West Virginia) and 23 participated in 2001 (http://www.cdc.gov/nccdphp/drh/pramstates.htm). PRAMS is the only survey with sufficient observations to permit a state-level analysis of pregnancy intention over time.[13]

B. Reduced form analysis

The reduced form analysis addresses the following question? Did cohorts exposed to legalized abortion experience a larger relative decrease or smaller relative increase in crime than cohorts unexposed to legalized abortion? The period-cohort diagram in Table 1 illustrates the basic approach. Year of birth is on the vertical axis and year of crime is along the top. Figures show the age of each cohort by year. As noted, abortion laws in five states and the District of Columbia, what I have referred to as the "repeal states", changed dramatically between late 1969 and 1970. The result was *de jure* or *de facto* legalization in repeal states almost three years prior to national legalization. The shaded portion in Table 1 highlights the cohorts that were exposed to legalized abortion prior to *Roe v Wade*. I limit the analysis to 15- to 24-years-olds because the Uniform Crime Reports records arrests by single year of age for this group only.

I take two approaches to analyzing the data. In the first, I examine arrest and homicide rates by single year of age. Thus, I compare the change in violent crime arrest rates of 18-year-olds in repeal states between 1971 and 1973—a period during which abortion was legal—to arrest rates of 18-year-olds also born between 1971 and 1973 in non-repeal states, where

[13] The National Longitudinal Survey of Youth (NLSY) and the National Survey of Family Growth (NSFG) ask women that give birth about pregnancy intention. Both surveys are two small for an analysis of state abortion rates and pregnancy intention over time. Moreover, the NSFG does not provide state identifiers.

abortion was illegal. Comparisons by single year of age allow for relatively precise assignment of those exposed and unexposed to legalized abortion in utero. A disadvantage is that data are not race-specific. A second disadvantage is that homicide is relatively rare, especially among younger teens. Thus, a number of observations are dropped in a semi-logarithmic specification.[14] The regression model is specified as follows:

$$LnC_{ajy} = ß_0 + ß_1(Repeal_j * Y7173_y) + ß_2(Repeal_y * Y7476_y) + ß_3(Repeal_j * Y7779_y) +$$

$$\sum ß_{4k}(age_a * state_j) + \sum ß_{5m}(age_a * year_y) + e_{ajy} \qquad (1)$$

where *LnC*$_{ajy}$ is the natural logarithm of the crime rate for age group, a, in state, j, and year of birth, y; *Repeal* is a dummy variable that is one for repeal states; *Y7173, Y7476* and *Y7779* are dummy variables for cohorts born in the designated years. The omitted category includes the birth years 1968-1970. Thus β_1, the coefficient on the interaction of *Repeal* and *Y7173,* measures the change in crime in repeal states relative to non-repeal states between 1971-73 and 1968-70. The identifying assumption is that the change in arrests or homicide rates in non-repeal states is a good counterfactual for changes in repeal states. The coefficients on the other two interaction terms, β_2 and β_3, measure the effect of national legalization. If abortion lowers crime, then *Roe v. Wade* should bring about a relative improvement in crime rates among non-repeal states. Thus, β_2 and β_3 should tend to zero depending on the speed of adjustment (Gruber, Levine and Staiger 1999). I also include dummy variables for all age and state interactions as well as age and year of birth interactions, as represented by the last two terms of equation (1). The specification is identical to that of Donohue and Levitt (2001) with the important difference that I have included categorical variables to measure differential exposure to legalized abortion instead of the actual abortion ratio.

[14] I tried to estimate count data models but the specifications included over 600 dummy variables and would not converge.

The second approach to the data uses within-state comparison groups to identify the effect of abortion legalization on homicide and arrest rates. Ideally, one would like a cohort close in age, unexposed to legalized abortion, but exposed to similar period affects that also affect crime. As Table 1 makes clear, such comparisons are possible. Consider, for instance, 18-year-olds born in 1969 and 1971 in repeal states. They are a "treatment" group since their exposure to legalized abortion changes between 1969 and 1971. A comparison group is 19-year-olds born in 1968 and 1970 also from repeal states. Both groups come of age in 1987 and 1989 and thus face the same period effects, but only the 18-year-olds have been exposed to legalized abortion in utero. Similar comparisons can be made between 19- and 20-year-olds. Again, the change in crime among 19-year-olds born in 1969 and 1971 is compared with the change in crime among 20-year-olds born in 1968 and 1970. As before, both the 19- and 20-year-olds are exposed to same period effects in the years 1988 and 1990. Equivalent comparisons can be made between 20- and 21-year-olds, 21- and 22-year-olds, up through 23- and 24-year-olds. The same set comparisons can be conducted for cohorts from non-repeal states. Finally, the relative changes in repeal states can be compared with those in non-repeal states to form a difference-in-difference-in-difference estimate (Gruber 1994; Ludwig 1998). In essence, I am using changes among the unexposed age group to "detrend" the change in crime among the exposed group within repeal and non-repeal states. I then compare the "detrended" change in crime among, for example, 18-year-olds in repeal states with the "detrended" change in crime among 18-year-olds in non-repeal states. The objective, of course, is to net out state-specific changes in crime due to factors specific to repeal and non-repeal states before making comparisons across states. The identifying assumption is that the relative change in the comparison group is a good counterfactual for the change that would have been observed in the exposed group had abortion not been legalized.

I perform a similar difference-in-difference analysis of race-specific homicide rates for teens 15 to 19 years of age and young adults ages 20 to 24. Almost all teens in repeal states that come of age between 1989 and 1990 were born after 1970 and thus were exposed to legalized abortion in utero (see Table 1). By contrast, in 1989-90 all young adults, ages 20 to 24, were born prior to 1971 and were unexposed to legalized abortion. Consequently, changes in crime between 1989-90 and 1985-86 among young adults in repeal states capture variation unrelated to legalized abortion and serve as a means of eliminating period effects among teens.

Only homicide data are race-specific and population data by year, state and race are available only in five-year groupings. Consequently, I lose some precision in the assignment to exposed and unexposed groups as compared with the analysis by single year of age. However, the pattern of homicide varies significantly by race and most likely reflects disparate responses to the crack epidemic. Since my objective is to eliminate period effects, stratification by race enhances the credibility of the comparison groups. Another advantage of the more aggregate age groupings is that I lose fewer observations because of zero homicide rates. The relevant regression model can be written as follows:

$$\mathrm{LnC}_{ajt} = \beta_0 + \beta_1 \mathrm{Teen}_a + \beta_2 (\mathrm{Repeal}_j * \mathrm{Y8990}_t) + \beta_3 (\mathrm{Teen}_a * \mathrm{Y8990}_t) + \beta_4 (\mathrm{Teen}_a * \mathrm{Repeal}_j)$$

$$\beta_5 (\mathrm{Teen} * \mathrm{Repeal} * \mathrm{Y8990}) + \mathrm{X}_{jt}\,\mathrm{p} + \sum ?_j \mathrm{S}_j + \sum ?_t \mathrm{Y}_t + \mathrm{e}_{ajt} \qquad (2)$$

where LnC_{ajt} is the natural logarithm of the homicide rate for age group, a, in state, j, and year, t; *Repeal* is a dummy variable that is one for repeal states; *Y8990* is a dummy variable for the designated years. The omitted category includes the years 1985-86.[15] State and year effects are represented by S_j and Y_t, and $\mathbf{X_{jt}}$ is the matrix of control variables used by Donohue and Levitt (2001). The DDD estimate is β_5, which measures the proportionate change in homicide rates

[15] For notational convenience, I omitted the interactions among the 1987-1988 period, teen and repeal state dummy variables.

before and after exposure to legalized abortion among teens relative to young adults in repeal relative to non-repeal states. If abortion lowers crime, then β_5 should be negative.

IV. Results

A. Abortion, fertility and unintended childbearing

The first set of results pertains to the relationships among abortion, fertility, and unintended childbearing. Figure 1 displays time-series of fertility and resident abortion rates for repeal and non-repeal states between 1961 and 1985.[16] The rates are births or abortions per 1000 women 15 to 44 years of age. The first observation is that the difference in fertility rates between repeal and non-repeal states is small (about 6 births per 1000 women) and relatively constant between 1961 and 1970. The gap widens after 1970 and is essentially closed by 1976 (Levine et al. 1999). However, the gap in abortion rates between repeal and non-repeal states is much larger, approximately 18 abortions per 1000 women, and does not diminish after 1975. Importantly, the discrepancy in abortion rates between repeal and non-repeal states is not reflected in a difference in fertility rates after 1975. If the abortion rate captures unintended pregnancies that would have become unintended births, then one would expect to observe a greater difference in fertility rates between the two groups of states.

One explanation for the discrepancy is that abortion is used more frequently as a substitute for contraception in repeal states because the cost and stigma associated with abortion are less. Thus, the rate of unintended childbearing may be similar across states despite different rates of abortion. Another interpretation consistent with the time-series patterns in Figure 1 is that abortion affects only the timing of births and not completed family size. Thus, the initial decline in fertility rates associated with early legalization represents a shift in the timing of births

[16] I use resident birth and abortion rates. Data on resident abortion were constructed from the Centers for Disease Control (1971, 1972, 1973), Henshaw, Forrest, Sullivan and Tietze (1981) and Henshaw and Van Vort (1992).

from adolescence to the early twenties, from the early twenties to the later twenties, and so on. This would preserve Donohue and Levitt's argument that states with greater increases in abortion had fewer mistimed births and lower subsequent crime rates. However, if Donohue and Levitt are correct, we still should observe an effect of legalized abortion on teen fertility rates in the period after *Roe,* since no age group delays childbearing into adolescence.

As a further examination, I estimated a series of regressions with total and teen state fertility rates as the dependent variable. In the first model, I regressed fertility rates on a set of indicator variables for interactions between repeal states and periods before and after *Roe*. In the second set of models, I regressed fertility rates on the abortion rate. The point is to estimate the simple correlation between abortion and fertility adjusted for state and year fixed effects.[17] Results are shown in Table 2. Columns (1) and (4) largely replicate the results in Levine et al. (1999). Since the dependent variable is in logs, the coefficient on Repeal*1971-1973 indicates that total fertility rates fell 6.5 percent and teen fertility rates 16 percent more in repeal relative to non-repeal states between 1971 and 1973. Columns (2) and (5) presents the results of a regression of log fertility rates on log abortion rates. The interaction term between repeal states and abortion rates allows the relationship between abortion and fertility to vary between repeal and non-repeal states. The interaction terms are positive; they indicate that in repeal states, a one-percent increase in the abortion rate is associated with .21 percent *increase* in the fertility rate of all women and a .26 increase in fertility rate of teens.[18] In non-repeal states the relationship between abortion and fertility is positive but statistically insignificant. If I limit the regression to the post-*Roe* period, after which abortions were measured more accurately, the positive coefficients for repeal states become even larger [columns (3) and (6)]. The direct

[17] Actually I used a two year moving average of the abortion rate to account for the 6 month difference in the length of pregnancy between an abortion and birth.

[18] These estimates are obtained by summing (.018 and .193) for all women and (.026 and .233) for teens.

association between abortion and teen fertility rates is inconsistent with the story that states with higher abortion rates have lower rates of unintended childbearing. Even if abortion had no effect on completed fertility and only affected timing, we should still see a negative correlation between abortion and teen fertility.

Despite a positive or insignificant relationship between abortion and fertility rates, one might argue that births in states with greater abortion rates are more favorably selected, or more intended (Joyce and Grossman 1990). Indeed, Donohue and Levitt's argument is essentially that legalized abortion increases the aggregate "intendedness" of births. Thus, states with comparatively high abortion rates should have a smaller proportion of births from pregnancies described as unintended. To provide some insight as to the relationship between abortion and births from unintended pregnancies, I regressed dichotomous indicators of pregnancy intention on state abortion rates with data from the Pregnancy Risk and Assessment Monitoring System or PRAMS (see Section IIIA). Results are shown in Table 3. In columns (1) and (2) the dependent variable is one if a woman described her pregnancy as unintended (mistimed or unwanted) and zero if intended. In columns (3) and (4) the dependent variable is one if the pregnancy was unwanted and zero otherwise. Each model includes controls for age, race and year and half the specifications include state fixed effects. The association between abortion and unintended pregnancy is positive, although statistically insignificant in the specification with state fixed effects. There is a negative association between the abortion rate and an unwanted pregnancy in the model with fixed effects, but it is measured imprecisely.[19]

[19] There are two caveats to this exercise. First, PRAMS is a survey of women that gave birth and thus is not the appropriate counterfactual for aborted pregnancies. Second, the data pertain to 1988-1997 whereas Donohue and Levitt use abortion data from 1973-1985. Abortion rates, however, are highly serially correlated. Donohue and Levitt (2001) report the 10-year correlation to be .91. Moreover, unintended births averted are unobserved. Thus, such exercises are one of the few ways available of assessing where changes in abortion rates are correlated with changes in births described as unintended.

Although a more developed model is necessary to sort out the endogenous relationships among abortion, pregnancy intention, and fertility, the lack of a negative association between reported legal abortion and fertility or reported abortion and unintended pregnancy calls into question Donohue and Levitt's identification strategy. The association between abortion and crime reported by Donohue and Levitt is obtained from the correlation of crime rates between 1985 and 1997 and abortion ratios between 1970 and 1985. Yet the authors have no data on abortion prior to *Roe*; moreover, the abortion rate appears unrelated to fertility or unintended pregnancy in the period after *Roe*. The latter point is significant because unless states with relatively high abortion rates have lower rates of unintended childbearing, the association between crime and abortion may be spurious. What is clear from the literature, however, is that the dramatic and largely unanticipated change in abortion availability in repeal states between 1971 and 1973 had a significant impact on fertility. If a relationship between legalized abortion and crime exists, then fertility changes in the period prior to *Roe* would seem a more credible source of identifying variation.

B. Reduced-form analysis

1. Time-series evidence.

Figures 2a-6d show the natural logarithm of arrest and homicide rates by single year of age stratified by repeal (R) and non-repeal states (NR).[20] Along the horizontal axis is the birth cohort. Thus, in Figure 2a the natural logarithm of the violent crime arrest rate per 1000 teens 17 years of age born between 1968 and 1970 was approximately 1.6 in non-repeal states, or, in levels, about 5.0 arrests per thousand 17-year-olds.[21] As shown in Figure 2a, arrest rates for

[20] I show the log of arrest and homicide rates in order to facilitate comparison with the regression results to follow.

[21] To convert cohorts into periods simply add years of age to cohort years. Thus, the arrest rate for 17-year-olds from the 1968-1970 birth cohort is observed between 1985 and 1987.

violent crime among teens 16 and 17 are higher in repeal relative to non-repeal states and they rise in each group of states with more recent birth cohorts. The comparison I wish to emphasize is the change in crime in repeal relative to non-repeal states between the 1968-70 and 1971-73 birth cohorts. As I have argued, this period offers the most convincing evidence of a relative fertility shock associated with legalized abortion. Figure 2a, however, offers little visual evidence that the change in crime was differentially affected by legalized abortion. For both 16- and 17-year-olds, the increase in violent crime arrest rates was slightly steeper between 1968-70 and 1971-73 in repeal than in non-repeal states.

A similar pattern is characteristic of arrest rates for violent crime among teens 18 and 19 years of age (Figure 2b). Again, there is little to suggest a differential change in arrest rates between 1968-1970 and 1971-1973 in repeal relative to non-repeal states. For older age groups, however, differences begin to emerge. The increase in the logarithm of arrest rates for young adults 20 and 21 years of age born between 1968-70 and 1971-73 in non-repeal states clearly exceeds that of repeal states (Figure 2c).

Except for property crime arrest rates (Figures 3a-3d), two patterns emerge from the remaining crime categories that are similar to those for violent crime. First, among teens crime always rises in both repeal and non-repeal states between the 1968-70 and 1971-73 birth cohorts (Figures 4a-4b, 5a-5b, 6a-6b). Whether crime, as proxied by arrest and homicide rates, rises more slowly in repeal states is often difficult to discern. Second, for young adults, especially those 22 and older, the level of crime is lower for the 1971-73 birth cohort as compared with the 1968-70 birth cohorts and the relative decline between these periods is generally greater among cohorts born in repeal relative to non-repeal states.

Thus, visual inspection of arrest and homicide rates offers mixed support for the hypothesis that legalize abortion lowers crime: exposure to legalized abortion between 1971-73 appears to have had little impact on the crime rate of teens, but more substantive effects on the decline among young adults. The inconsistency, however, is difficult to reconcile with strong cohort effects associated with legalized abortion. In other words, why should exposure to legalized abortion have no effect on teens but large effects on young adults, all of whom are members of the same cohort. What is not obvious from the plots, but is shown in subsequent graphs (Figures 8-11), is that crime peaks in roughly the same years for all teens and young adults in both repeal and non-repeal states, a pattern more consistent with strong period effects such as the decline in crack/cocaine markets.

2. Reduced form regressions by single year of age

Table 4 presents results from the estimation of equation (1) for 15- to 24-year-olds. All dependent variables are expressed as natural logarithms.[22] I display only the coefficients on the interaction terms [β_1- β_3 in equation (1)]. They should be interpreted as the proportionate difference in crime between the 1968-70 cohorts and subsequent cohorts in repeal relative to non-repeal states. If exposure to legalized abortion is associated with decreases in crime, then arrest rates and homicide rates should fall or increase less among 1971-73 cohorts in repeal relative to non-repeal states. National legalization of abortion, however, should attenuate the relative advantage of cohorts in repeal states. As a result, the coefficients on the other two interactive terms, α_2 and α_3, should tend to zero (see Gruber, Levine and Staiger 1999).

The results in Table 4 restrict the effect of exposure to legalized abortion to be the same for teens and young adults, a restriction clearly at odds with Figures 2-6. I present them first because they are most consistent with Donohue and Levitt's results and they provide an important point of comparison when I relax this restriction. Estimates in Table 4 indicate that

[22] There are 4359 potential observations given 10 age groups, 51 states and the limitation that one had to be born between 1968-79 and be arrested or killed between 1985 and 1996. As shown in Table 3, the period-cohort diagram, 17-year-olds contribute 12 years of arrest rates. All other age groups contribute less.

arrest rates for violent crime, property crime and murder between the 1968-70 and 1971-73 birth cohorts fall or rise less by 3.2, 4.5 and 7.4 percent, respectively, in repeal relative to non-repeal states. Similar declines are observed for homicide victimization and offending rates (columns 4-6).[23] An unexpected result, however, is that the advantage associated with early legalization in repeal states often becomes greater for more recent cohorts. Indeed the relative decline in murder arrest rates (Table 4, column 3) among cohorts born between 1977-79 in repeal states is almost five times greater than the relative decline experienced by the 1971-73 cohorts.

Tables 5a and 5b display results in which the reduced form effects of legalized abortion on crime are allowed to vary by age and cohort. Coefficients in each column show the relative difference in arrest and homicide rates by birth cohorts and single year of age in repeal relative to non-repeal states. By and large, the estimates quantify the patterns observed in Figures 2-6. Consider the comparison of the 1971-73 and 1968-70 birth cohorts for teens 15-19 years of age. Only 12 of the 30 coefficients in Tables 5a and 5b are negative and none are statistically significant at the 5 percent level [columns (1), (4) & (7) in Tables 5a and 5b]. Results are the opposite among young adults ages 20 to 24: twenty-seven of the 30 coefficients in the lower half of columns 1, 4 and 7 are negative and over half are statistically significant. In addition, the magnitude of the effect of abortion legalization on homicide is often large, with relative declines of between 15 and 40 percent among cohorts in repeal relative to non-repeal states.

Estimates for the 1974-76 cohorts but especially the 1977-79 birth cohorts are also unexpected. As Gruber, Levine and Staiger (1999) argue, one would expect the coefficients for cohorts in repeal states born after *Roe v. Wade* to tend to zero as national legalization of abortion leads to relative decreases in crime in the non-repeal states, what the authors term "bounceback."

[23] Note also that I show homicide victimization rates by state of residence (column 4) and state of birth (column 6). The similarity of the coefficients suggests that migration from state of birth to state of residence may not be an important source of misclassification in the exposure measure.

Instead I find that the coefficients on older teens born between 1977-79 in repeal states are negative, large in magnitude, and statistically significant.

Taken together, the time-series plots and reduced-form regressions are inconsistent with a strong cohort effect. Why should teens exposed to legalized abortion respond differently from young adults? Why do differences between repeal and non-repeal states widen and not narrow with national legalization? It is important to recall that teens born between 1971-73 come of age between 1986 and 1992, a period of rapid increase in violent crime. Young adults ages 20 to 24 from the 1971-73 birth cohorts come of age largely after 1992, amidst the rapid decline in crime. Thus, when I restrict age effects to be same as in Table 4, the average effect for those 15 to 24 years of age is negative

To illustrate the sensitivity of the relationship to specific periods, I divided the study period into two segments, 1985-1990 and 1991-1997. With data provided by Donohue and Levitt, I re-estimated their index crime regressions separately for each of the two periods (results not shown). For the 1985-1990 period the coefficient on the effective abortion ratio is 0.276 (t-ratio=4.17) in the murder regression, 0.017 (t-ratio=0.37) in the violent crime regression and -0.033 (t-ratio =-1.85) in the property crime regression. Thus, except for property crime, there is no evidence that abortion lowers crime between 1985 and 1990, a finding similar to the results in Tables 5a and 5b above. For the 1991-1997 period, the results change dramatically. The coefficient on effective abortion ratio in the murder equations becomes -0.338 (t-ratio=-6.39); the coefficient in the violent crime becomes -0.209 (t-ratio=-6.03) and coefficient in the property crime regression becomes -0.186 (t-ratio =-5.49).

In an analysis of total crime rates, the relationship between the effective abortion ratio and index crimes should be weaker before 1990 than after because a relatively small proportion of all criminals would have been exposed to legalized abortion. Therefore, as a further test of the sensitivity of their estimates to the period before and after 1990, I re-estimated Donohue and Levitt's regressions of log arrests by single year of age for teens and young adults. In these regressions, the authors do not use the effective abortion ratio, but instead assign each age to the state abortion ratio that existed the year prior to birth. For instance, arrests for 15-year-olds in New York in 1990 are assigned the abortion ratio in New York in 1974. This is a better test of temporal homogeneity of effects since there is substantial within-state variation in abortion in the early 1970's. I re-ran the regressions and limited the sample to the years 1985-90 (results not shown). The coefficient on the abortion ratio in the equation for violent crime arrests is 0.020 (t-ratio=3.17), -0.028 (t-ratio=-4.99) in the equation for property crime arrests, and 0.042 (t-ratio=3.34) in the equation for murder arrests. Thus, results for violent crime are opposite of what Donohue and Levitt obtain for the full sample and the opposite of what would be expected for murder.

The discrepancy in the association between exposure to legalized abortion and crime before and after 1990 is more consistent with a strong period effect such as the rise and decline of crack markets than with a significant cohort effect. Nevertheless, the difference-in-difference strategy represented by equation (1) should identify effects of legalized abortion if period effects are the same between repeal and non-repeal states. If, however, the explosion of crack use hit difference states at different times, as evidence suggests, then such time-varying effects would not be eliminated by the difference-in-difference strategy. For instance, data from the Drug Use Forecasting Program (DUF) and the Drug Awareness Warning Network (DAWN) suggest that the arrival and duration of crack/cocaine use varied by city. In both New York and Los Angeles major crack/cocaine use began relatively early, about 1985, but also peaked sooner than in most cities (Golub and Johnson 1997; Cork 1999; Grogger and Willis 2000). The empirical challenge, therefore, is to find other means of separating period from cohort effects in the absence of consistent data on crack use by year and state.

3. *Within-state comparison groups by single year of age*

An alternative means of adjusting for period effects is to use a within-state comparison group. In this analysis I compare changes in crime by single year of age between the 1971 and 1969 birth cohorts with changes in crime among the 1968 and 1970 birth cohorts separately within repeal and non-repeal states. I then contrast these difference-in-differences between repeal and non-repeal states. Only the 1971 cohort in repeal states was exposed to legalized abortion and yet, all cohorts experienced the same period effects. If, for instance, the spread of crack and gang violence differed in timing and scope between repeal and non-repeal states, then a within-state comparison group will adjust for such confounding.

The advantage of using 1969 and 1971 as the exposure period is illustrated in Table 6. There is an 11 percent drop in fertility rates among teens and a 5 percent drop in the fertility rate of all women in repeal as compared to non-repeal states between 1969 and 1971. Moreover, the change between 1971 and 1969 dominates the total change between 1973 and 1969. The relative fertility rate change from 1971 to 1969 among all women in repeal states is 82 percent of the total change that occurs between 1969 and 1973. This is plausible since legalization occurred suddenly in New York and California. As referral networks developed, more women traveled to repeal states for abortion in 1971 and 1972 as compared to 1970, which likely diminished the relative impact of the law on fertility (Tietze 1973; Centers For Disease Control 1972, 1973). The important point from an analysis of abortion and crime is that the changes in relative fertility rates between 1969 and 1971 may be the most clear cut source of identifying variation available in unintended childbearing with which to test for its effect on crime.[24]

[24] One limitation of such a tight pre- and post-exposure period is that an 18-year-old killed or arrested in 1989, for example, could have been born in January, 1970 if he/she were 18 years and 12 months when arrested or killed in January of 1989. Such

The results in Table 7 show proportionate changes in crime for cohorts exposed relative to cohorts unexposed to legalized abortion. For each crime, the DD estimates contrast changes in crime between the two age groups within repeal states only; the DDD estimates compare relative changes between repeal and non-repeal states.[25] There are potentially 1224 observations given data by age, year of birth, and state. However, due to missing data or zero homicides, actual sample sizes vary from 1146 for arrest rates for violent and property crime to 997 for murder arrest rates. In each column the younger age group is from the 1969 and 1971 birth cohorts and the older age group is from the 1968 and 1970 birth cohort. If legalized abortion is associated with lower crime and arrest rates, then the DD and DDD estimates should be negative.

I find that except for homicide among 20- and 21-year-olds (column 3), there is little to suggest that the change in crime among those exposed to legalized abortion differed from those unexposed. For instance, arrest rates for violent crime in repeal states fell one percent more among 18 relative to 19 year-olds between 1989 and 1987 (column 1). When adjusted for similar changes in non-repeal states, the relative decline among 18-year-olds was 7.5 percent. Neither change was statistically significant. Indeed, there is not one statistically significant change in any arrest rate for any age group. Another noteworthy result is that changes in homicide and murder arrest rates among young adults 21- to 23-year-olds are almost always smaller than the 1968-70 to 1971-73 changes in Tables 5a and 5b. This suggests that the use of comparison groups within repeal and non-repeal states nets out shared period effects more effectively than a specification with only state and year fixed effects.

There are large negative and statistically significant effects of abortion legalization on homicide victimization and offending rates among 20- relative to 21-year-olds (column 3, lower

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misclassification would tend to bias estimates downwards. Thus, I also perform analyses in which the pre- and post-exposure period is from 1969 to 1972 and the age difference between the "treatment" and comparison group is two years instead of one. Thus 18-year-olds are compared to 20-year-olds, 19-year-olds are compared to 21-year-olds, etc.

half). These estimates are inconsistent not only with changes in homicide among other age groups, but also are inconsistent with results for arrest rates among 20- relative to 21-year-olds (top half of column 3). For a closer look at crime among 20- and 21-year-olds, I plotted the natural logarithm of arrest and homicide rates for 20- and 21-year-olds in Figures 7a-7f. Year of birth is on the horizontal axis and the log of arrest and homicide rates is on the vertical axis. Figures 7a-7c show trends in arrest rates for the index crimes of violence, property and murder, respectively. As is apparent, there is no visible difference in arrest rate trends of 20- and 21-year-olds born between 1968 and 1971 and no differential break in trend associated with the legalization of abortion in 1971. There is, however, a substantial jump in homicide rates among 21-year-olds in repeal states between the 1968 and 1970 cohorts (Figures 7d-7f), which is primarily responsible for the large negative DD and DDD estimates for homicide among 20-year-olds. Moreover, when I compare 20- to 22-year-olds in analyses with 1969 and 1972 as the pre- and post-exposure period, I find no effect of abortion legalization on the homicide rates of 20-year olds (results not shown).[26] Given the lack of corroboration among the other series, the decline in homicide rates among 20- relative to 21-year olds is best interpreted as an anomalous finding.

4. With-state comparisons by age and race

Figures 8-11 show race-specific homicide offending and homicide victimization rates for teens and young adults by race, year, and state. Two observations are salient to the DD strategy as represented by equation (2). First, there are significant racial differences in the level and time path of homicide. Homicide rates among blacks rise from 1985 in both repeal and non-repeal states. Homicide rates among whites don't begin to increase rapidly until 1988 in repeal states,

[25] To save space I show only the DD estimates for repeal states. The DD estimates for non-repeal states are simply the DD for repeal minus the DDD.

and remain relatively flat in non-repeal states. Second, trends in homicide, especially homicide offending, among young adults and teens within repeal and non-repeal states are quite similar. Both tend to rise, peak and fall at roughly the same time and rate (Figures 10 and 11).

In an analysis of the years 1985-1990, teens in repeal states are the "treatment" group and young adults the comparison group. [27] A contrast of crime rates between 1990-91 and 1996-97 reverses the experiment. Specifically, by 1990 all teens in repeal states had been exposed to legalized abortion whereas only a tenth of young adults have been exposed.[28] By 1995, all young adults in repeal states had been exposed. Thus, the DD for repeal states between 1996-97 and 1990-91 subtracts the change in crime among teens from the change in crime among young adults. Since teens experience no change in exposure during this time, they serve as the comparison group for young adults. I estimate a similar set of DDs for the non-repeal states, although the contrast in exposure is not as great as it is in the repeal states.[29]

Estimates of the DD and DDD from equation (2) are presented in the top panel of Table 8. The analogous estimates for the comparison of young adults and teens between 1997-96 and 1990-91 are shown in the lower panel. There are potentially 612 age/state/year observations for each racial group between 1985 and 1990 and 816 observations for the 1990-1997 comparisons. Due to missing data or lack of reported homicides, available observations are less. Each

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[26] In general, comparisons based on 1969 and 1972 as the pre- and post-exposure period are largely consistent with results reported in Table 7.

[27] Recall that all teens in 1985 had been born before 1971 and thus were unexposed to legalized abortion in utero; by 1990, all teens had been born after 1971 and therefore had been exposed to legalized abortion. By contrast, none of the young adults ages 20 to 24 between 1985 and 1990 had been exposed to legalized abortion in utero, which makes them a relevant comparison group.

[28] Refer to Table 1. Only 20-year-olds in 1991 were exposed to legalized abortion between 1990 and 1991. Given 5 age groups and two years there are 10 possible "cells" of exposure to legalized abortion between 1990 and 1991.

[29] Again refer to Table 1. Between 1990 and 1991, 5 of the possible 10 teen age groups had been exposed to legalized abortion. Recall that exposure in non-repeal states begins with the 1974 birth cohort. Thus between 1990-91 and 1996-97 the increase in exposure to legalized abortion is greater among young adults than among teens in non-repeal states, but the contrast is not as great as in repeal states. Equation (2) can easily be modified to reflect the analysis of this "second experiment."

specification includes the same set of control variables as were used by Donohue and Levitt (2001).

Estimates from the first-three rows of Table 8 argue against any reduction in homicide from exposure to legalized abortion. The DD estimates in rows 1 and 2 indicate that homicide rates increased relatively more among teens than young adults between 1985 and 1990. Seven of eight DD estimates are statistically significant. The DD estimates in row 2 are instructive because they reflect the finding reported elsewhere that the upsurge in homicide was relatively greater among teens than young adults (Blumstein 2000). The DDD estimates in row 3 compare the relative changes in crime among teens in repeal and non-repeal states after eliminating state-specific (repeal vs. non-repeal) period effects. If abortion lowers crime, then estimates in Table 3 should be negative. There is no evidence that crime fell more among teens exposed as compared with unexposed to legalized abortion.

Results for young adults are displayed in the lower panel of Table 8. In this analysis teens serve as the comparison group since their exposure in repeal states did not change. Row 4 shows the DD estimates for repeal states. The estimates are small and statistically insignificant for both whites and blacks. The DD estimates for non-repeal states are in row 5. The DD estimates in row 5 and the DDD estimates in row 6 should be interpreted more cautiously. The “experiment” in non-repeal states is less sharp between 1990-91 and 1996-97, since teens--the comparison group--went from partially exposed to fully exposed to legalized abortion over this period (see footnote 29). The DD estimates for whites in row 5 are large and statistically significant, suggesting that abortion had an important differential impact on homicide among young white adults. For blacks, the DD estimates suggest legalized abortion had no effect. The DDD estimates show a positive and insignificant effect of legalized abortion on homicide rates.

What overall conclusion can be drawn from the reduced form estimates in Tables 4-8? The results in Table 4 that associate exposure to legalized abortion with arrest and homicide rates for 15- to 24-year-olds collectively are largely consistent with Donohue and Levitt's findings: states that repealed abortion prior to *Roe v. Wade* experienced earlier decreases in crime than non-repeal states. However, this initial association is not robust to a more refined analysis by age, race and sub-periods. As Tables 5a and 5b indicate, there is no association between exposure to legalized abortion and arrest and homicide rates among teens. Moreover, there is no association between legalized abortion and crime among young adults 21 to 23 years of age when I use within-state comparison groups. Finally, I find no association between the legalization of abortion and crime among black teens and young adults, groups for whom the demographic impact of abortion was greatest. Thus, my conclusion at this juncture is that the association between legalized abortion and crime as reported by Donohue and Levitt is probably spurious, due to the inability to control for changes in crack use and its spillover effects.

C. Crime and unintended births averted

To this point the analysis of crime and abortion has relied on a reduced-form comparison of crime and arrest rates in repeal relative to non-repeal states. Donohue and Levitt, however, emphasize the direct correlation between lagged abortion ratios and crime in an effort to measure the effect of unintended births averted. Yet, it is not apparent that variation in abortion ratios identifies changes in unintended childbearing, especially in the period after *Roe*. What is clear from the literature is that the legalization of abortion in 1970 is strongly associated with a relative decline in fertility rates in repeal relative to non-repeal states. As I have argued, this fall in fertility is a less ambiguous source of identifying variation for unintended births averted than is variation in reported abortions. Moreover, fertility rates are more accurately measured than abortion rates especially in the 1960's and early 1970's; they also are available by race.

Accordingly, I use the DDD framework from above and regress a cohort's race-specific homicide rate in period t on the race-specific fertility rate in year t-a, and the interaction of the fertility rate and exposure to legalized abortion in year t-a, where a is the age of the cohort at time t. The objective is to difference out the underlying relationship between fertility and crime among groups unexposed in order to capture the differential effect of a fall in fertility among groups exposed. To illustrate, consider Equation (3).[30]

$$\mathrm{LnC}_{ajt} = ß_0 + ß_1 \mathrm{LnB}_{ajt-a} \ldots + ß_8 (\mathrm{LnB}_{jt-a} * \mathrm{Repeal}_j * \mathrm{Y8990}_t * \mathrm{Teen}_a) + \mathbf{X}_{\mathbf{jt}} \mathbf{?} + \mathrm{S}_j + \mathrm{T}_t + \mathrm{e}_{ajt} \quad (3)$$

Ln C_{ajt} is the natural log of homicide in state j and year t and age group a; Ln B_{ajt-a} is the natural log of the fertility rate in period t-a.; Teen_a is a dummy variable for adolescents; Repeal_j is a dummy variable for repeal states; and $\mathrm{Y8990}_t$ is a dummy variable for the years 1988-90. The fertility rate is lagged 18 years for teen homicide and 22 years for young adult homicide. The variables $\mathbf{X}_{\mathbf{jt}}$ are the vector of covariates used by Donohue and Levitt (2001) and S and T control for state and year fixed effects.

The DDD is estimated by β_8, which shows the percentage change in crime for every one percent increase in the fertility rate before (1985-86) and after (1989-90) exposure to legalized abortion among teens relative to young adults in repeal relative to non-repeal states. If abortion lowers crime, then β_8 should be negative. The rationale is that legalized abortion lowers the fraction of births in any cohort that are unintended, which should decrease a cohort's criminal behavior as teens. In other words, the relatively greater decline in fertility rates between 1971 and 1973 among those exposed to legalized abortion captures the impact of differential changes in unintended births averted on crime rates.

[30] The full set of interactions has been suppressed for convenience.

The results in the top panel of Table 9 show the DD and DDD estimates from the estimation of equation (3). The lower panel shows results from the same exercise but for the period 1990-1997, when young adults were the exposed group and teens the comparison group. There is little evidence of any association between unintended childbearing—as proxied by differential changes in fertility—and homicide. The DDD estimates in row 3 are small and statistically insignificant. The results for young adults are equally unsupportive of a relationship between abortion and crime.

D. Sensitivity analyses

1. Functional form

Donohue and Levitt's conclusion that legalized abortion lowered crime is based primarily on regressions of the natural log of the total crime rate on the level of the effective abortion ratio. The specification implies an exponential relationship between crime rates and abortion. However, we are really interested in the relationship between crime and unintended births averted, which is unobservable. By entering the level of the abortion ratio in a regression of log crime rates, Donohue and Levitt (2001) assume the relation between abortion and unintended births averted is also linear. Other functional forms are clearly possible.[31]

To assess the sensitivity of their estimates to an alternative specification , I re-estimated Donohue and Levitt's regressions as presented in Table 4 of their paper. I substituted the natural logarithm of the effective abortion ratio for the level of the abortion ratio. The results are displayed in Table 10. The first row of coefficients replicates the results in Donohue and Levitt

[31] Let C be the crime rate, U the rate of unintended childbearing and A the abortion rate. Donohue and Levitt run Ln C = a_1*U where (a_1>0). However, U is unobserved. Consider, U = b_1*LnA where (b_1<0). This implies diminishing returns to abortion, which would be plausible if substitution of abortion for contraception increased as the relative price of abortion fell. Substituting for U in the crime equation yields Ln C = c_1*Ln A where c_1=a_1b_1.

(2001). The second row of coefficients is for the log of the effective abortion rate. These coefficients are *positive* in every case and statistically significant in five out of six regressions.

An important limitation of using the natural logarithm of the effective abortion ratio is that small absolute increases can be very large relative increases if the initial level is low. The third row of Table 10 shows the weighted mean, standard deviation and the minimum and maximum value of the effective abortion ratio in the sample. There is clearly an extreme range of values for the effective abortion ratio. To lessen the impact of such extreme relative changes, I stratified the sample by repeal and non-repeal states and I restricted it to the years 1991-1997. This increased the means, lowered standard deviations, and tightened the range between the minimum and maximum. I then re-estimated the regressions for the sub-sample with both the level and the logarithm of the effective abortion ratio. The results in the lower two panels of Table 10 mirror those from the full sample: a negative relationship between the natural log of the crime rate and the level of the effective abortion ratio becomes positive in every instance when the natural logarithm of crime is regressed on the natural log of the effective abortion ratio.

2. Travel and migration

Effects of abortion legalization on crime will be biased if women in non-repeal states traveled to repeal states to obtain abortion.[32] As a simple check, I created a category of border states that consisted of any state that shared a border with California, New York and Washington, D.C. Alaska and Hawaii were obviously inaccessible and Washington State had a three-month residency requirement. I re-estimated equation (1) for the years 1968-1976. If women in border states were more likely to obtain an abortion than women in non-border states, then the coefficient on the interaction term, *Repeal *1971-73*, should become more negative; in addition, the coefficient on the interaction of the border state dummy and the years 1971-73

[32] The magnitude and even direction of the bias depend on the extent of travel and the probability of criminality among those that would have been born had travel for an abortion not occurred.

should also be negative but smaller in absolute value than the coefficient on *Repeal *1971-73.* The results were not consistent with this expectation (not shown). Indeed there was no consistent pattern to the results. For four outcomes, coefficients on the border state terms were positive and often statistically significant. For violent crime arrest rates the coefficient was more negative than the coefficient on repeal states and statistically significant.

Another source of bias is that most crime statistics are measured by state of residence whereas the indicators of abortion legalization measure exposure by state of birth. One useful aspect the NCHS victimization data is that they are available by state of birth as well as state of residence. The results in Tables 4, 5b and 7 show little meaningful difference between estimates of abortion and homicide victimization as measured by state of residence and by state of birth. I interpret this as some evidence that relocation to another state is not a significant source of bias.

3. Sample selection and state-year fixed effects

In all analyses, I dropped observations if there were zero homicides in a state-year-age cell. To assess the sensitivity of the estimates to this loss of data, I re-ran the regressions in Tables 4, 5a and 7 for violent crime and property crime arrest rates for the sub-sample of state-year-age cells that were available for murder arrest rates. There was little difference between these coefficients and those in Tables 4, 5a and 7 (not shown). The result is not surprising, since the regressions were weighted by the state population and the dropped observations tended to be younger age groups from small states.

I used comparison groups within repeal and non-repeal states to adjust for within-state trends in crime that may have differed between these two types of states. A less restrictive adjustment is to include state-year-of-birth (Table 7) or state-year (Table 9) interactions. The inclusion of state-specific trends had qualitatively little effect on the estimates, but did increase the standard errors (results not shown).

V. Conclusion

Donohue and Levitt (2001) present an intriguing association between the growth in abortion and the decrease in crime in the 1990's. The evidence I have presented does not support a causal interpretation of this association. The primary difference between Donohue and Levitt's approach and mine is one of identification. I have argued that the most useful source of identifying variation with which to uncover a relationship between the legalization of abortion and crime is a comparison of cohorts exposed and unexposed to the pre-*Roe* repeal of abortion laws between 1970 and 1972. There is substantial evidence that the pre-*Roe* legalization of abortion reduced fertility and by extension, unwanted childbearing. By contrast, Donohue and Levitt rely on the adjusted correlation between abortion ratios from 1970 to 1985 and crime rates from 1985 to 1997. However, abortion is poorly measured prior to *Roe;* moreover, it is more difficult to demonstrate an association between abortion and unintended childbearing in the period after *Roe,* which raises uncertainty as to what differential changes in abortion within states are measuring.

The other major difference between their study and mine is the effort to control for confounding from unobserved period effects such as crack and handgun use. I used the relative change in crime among comparison groups of similar age and race within repeal and non-repeal states to minimize confounding. The comparison groups were either unexposed to legalized abortion or experienced no change in exposure during the study period. The assumption was that variation in the crime rate of the comparison group was a reasonable counterfactual for what would have been observed among the "treatment" group had there been no legalization of abortion prior to *Roe.* Donohue and Levitt used controls for national trends to adjust for changes due to hard to measure factors that affect crime. However, the arrival and diffusion of crack varied by state, which limits the usefulness of national trends as a control for such confounding. In sum, even if one were to challenge my choice of data, selection of comparison groups, or argue for a particular functional form, the magnitude of the effects reported by Donohue and Levitt (2001), the inability to control for state-year effects, the fragility of their estimates, and the controversial policy implications indicate that further, more refined analyses are needed before causal inferences are drawn.

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Table 1. Period-Cohort Diagram

Year of Birth	Years of Crime Analyzed												
	1985	**1986**	**1987**	**1988**	**1989**	**1990**	**1991**	**1992**	**1993**	**1994**	**1995**	**1996**	**1997**
1968	17	18	19	20	21	22	23	24	25	26	27	28	29
1969	16	17	18	19	20	21	22	23	24	25	26	27	28
1970	15	16	17	18	19	20	21	22	23	24	25	26	27
1971		15	16	17	18	19	20	21	22	23	24	25	26
1972			15	16	17	18	19	20	21	22	23	24	25
1973				15	16	17	18	19	20	21	22	23	24
1974					15	16	17	18	19	20	21	22	23
1975						15	16	17	18	19	20	21	22
1976							15	16	17	18	19	20	21
1977								15	16	17	18	19	20
1978									15	16	17	18	19
1979										15	16	17	18

Table 2

Association Between the Fertility Rate, Abortion Legalization and the Abortion Rate U.S. 1961-1985

	Dependent Variable					
	Log Fertility Rate			*Log Teen Fertility Rate*		
	1961-1985	1971-1985	1974-1985	1961-1985	1971-1985	1974-1985
	(1)	(2)	(3)	(4)	(5)	(6)
Repeal x 1971-73	-.065*			-.159*		
Repeal x 1974-76	-.015			-.120*		
Repeal x 1976-89	.027*			-.053+		
Log abortion rate		.018*	.004		.026	.019
Log abortion rate*repeal state		.193*	.220*		.233*	.305*
Adjusted R^2	.94	.92	.93	.91	.87	.86
N	1275	765	612	1275	765	612

[a] All regressions include controls for state and year effects and all are weighted by the resident population of women 15 to 44 years of age in the state. Repeal is a dummy variable that is one for the states of Alaska, California, Washington D.C., Hawaii, New York and Washington all of which had completely or greatly liberalized abortion by 1971. The abortion and fertility rates are all resident abortions or births per 1000 women 15 to 44 years of age. The teen fertility rate is births to teens 15 to 19 years of age per 1000 teens of the same age.

* $p<.01$; + $p<.05$

Table 3

Association between Pregnancy Intention and the State Abortion Rates based on Pregnancy Risk and Monitoring System (PRAMS) 1988-1997

	Unintended Pregnancy=1		Unwanted Pregnancy =1	
Abortion rate x 100	.098 (.047)	.054 (.236)	.038 (.031)	-.137 (.157)
State fixed effects	No	Yes	No	Yes
Mean of dep var	.447		.119	
R-squared adj	.123	.124	.042	.043
N	105,988		105,988	

Data are from 11 states with the number of years from each state in parentheses: AL(6), AK(8), FL(5), GE(5), IN(2), ME(10), MI(4), OK(10), SC(6), WA(5), WV(10). Estimates were obtained by OLS. Coefficients (standard errors) have been multiplied by 100. All regressions are weighted and standard errors are adjusted for survey design. All models include dummy variables for age (4), race (2) and year (9). The abortion rate is the number of abortions by state of occurrence per 1000 women 15 to 44 years of age.

Table 4
Arrests, Arrest Rates, Murder Victimization Rates and Murder Offending Rates by Birth Cohort and Repeal and Non-repeal States, 1985-1996

	Ln violent arrest rates	*Ln property arrest rates*	*Ln murder arrest rates*	*Ln murder Victimization rates, resident*	*Ln murder offending rates*	*Ln murder victimization rates, birth state*
Birth cohorts	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Repeal*71-73	**-0.032**	**-0.045**	**-0.074**	**-0.067**	**-0.058**	**-0.081**
	0.021	0.015	0.038	0.039	0.041	0.039
Repeal* 74-76	**0.011**	**-0.035**	**-0.119**	**-0.064**	**-0.157**	**0.023**
	0.022	0.017	0.042	0.047	0.051	0.047
Repeal* 77-79	**-0.044**	**-0.065**	**-0.372**	**-0.165**	**-0.350**	**-0.053**
	0.032	0.029	0.062	0.082	0.075	0.087
N	4210	4213	3595	3768	3702	3755
R-square adj	0.919	0.916	0.930	0.796	0.757	0.768

Coefficients in bold (standard errors below) are relative changes in arrest and homicide rates for birth cohorts in repeal relative to non-repeal states. The omitted category is the 1968-70 birth cohort. All specifications include fixed effects for interactions of age and state as well as age and year of birth (see equation 1 in text). Standard errors have been adjusted for intra-class correlation within state and year of birth cells. There are 4539 possible cells: 10 age groups, 51 states and a varied number of age/year cells since the sample is limited to cohorts born between 1968 and 1979. There are missing observations either because states did not report in those years or there were no homicides or arrests in a particular age, state, year cell. All regressions have been weighted by the state population.

Table 5a

Arrest Rates by Single Year of Age and Birth Cohorts

	Violent Crime Arrest Rates			**Property Crime Arrest Rates**			**Murder Arrest Rates**		
	birth cohorts			birth cohorts			birth cohorts		
	1971-73	*1974-76*	*1977-79*	*1971-73*	*1974-76*	*1977-79*	*1971-73*	*1974-76*	*1977-79*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Age=15	**-0.016**	**0.131**	**-0.015**	**-0.025**	**0.020**	**-0.051**	**0.041**	**0.107**	**-0.235**
	0.069	0.067	0.070	0.029	0.032	0.038	0.181	0.188	0.179
Age=16	**0.101**	**0.168**	**0.049**	**0.014**	**0.057**	**-0.009**	**0.207**	**0.115**	**-0.106**
	0.046	0.041	0.044	0.034	0.035	0.045	0.098	0.111	0.111
Age=17	**0.084**	**0.099**	**0.046**	**0.034**	**0.103**	**0.025**	**0.097**	**-0.084**	**-0.219**
	0.040	0.035	0.042	0.035	0.035	0.037	0.088	0.086	0.098
Age=18	**-0.022**	**-0.073**	**-0.058**	**0.008**	**-0.007**	**-0.029**	**-0.042**	**-0.261**	**-0.225**
	0.035	0.033	0.044	0.037	0.035	0.055	0.075	0.078	0.077
Age=19	**-0.054**	**-0.069**	**-0.047**	**0.037**	**-0.069**	**-0.091**	**-0.135**	**-0.369**	**-0.259**
	0.042	0.035	0.073	0.038	0.037	0.075	0.072	0.057	0.073
Age=20	**-0.110**	**-0.034**		**-0.046**	**-0.083**		**-0.219**	**-0.211**	
	0.045	0.047		0.040	0.041		0.094	0.080	
Age=21	**-0.094**	**-0.045**		**-0.070**	**-0.108**		**-0.212**	**-0.294**	
	0.042	0.046		0.037	0.049		0.085	0.087	
Age=22	**-0.034**	**-0.008**		**-0.064**	**-0.165**		**-0.185**	**-0.051**	
	0.043	0.064		0.038	0.054		0.073	0.096	
Age=23	**-0.045**			**-0.087**			**-0.287**		
	0.038			0.044			0.079		
Age=24	**-0.056**			**-0.153**			**-0.093**		
	0.039			0.044			0.082		
R2	0.92			0.92			0.80		
N	4210			4213			3595		

Coefficients in bold (standard errors below) are relative changes in age-specific arrests rates for birth cohorts in repeal relative to non-repeal states. The omitted category is the1968-70 birth cohort. All specifications include fixed effects for interactions of age and state as well as age and year of birth. Standard errors have been adjusted for intra-class correlation within state and year of birth cells. All regressions have been weighted by the state population.

Table 5b

Murder Victimization and Offending Rates by Age, Birth Cohorts and Repeal and Non-Repeal States

	Murder Victimization State of Residence			**Murder Offending Rates**			**Murder Victimization State of Birth**		
	birth cohorts			birth cohorts			birth cohorts		
	1971-73	*1974-76*	*1977-79*	*1971-73*	*1974-76*	*1977-79*	*1971-73*	*1974-76*	*1977-79*
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Age=15	**0.162**	**0.239**	**0.204**	**0.101**	**0.095**	**-0.181**	**0.060**	**0.032**	**0.143**
	0.113	0.104	0.124	0.107	0.109	0.092	0.135	0.120	0.131
Age=16	**0.187**	**0.144**	**0.141**	**0.474**	**0.186**	**0.130**	**0.121**	**0.148**	**0.188**
	0.165	0.172	0.167	0.095	0.127	0.105	0.235	0.234	0.241
Age=17	**0.110**	**0.165**	**0.092**	**0.118**	**-0.019**	**-0.153**	**-0.026**	**0.116**	**0.164**
	0.129	0.094	0.114	0.113	0.099	0.122	0.112	0.098	0.132
Age=18	**0.047**	**-0.044**	**-0.150**	**-0.011**	**-0.178**	**-0.345**	**-0.084**	**-0.044**	**-0.015**
	0.095	0.091	0.123	0.081	0.078	0.119	0.100	0.102	0.128
Age=19	**-0.058**	**-0.123**	**-0.177**	**-0.088**	**-0.270**	**-0.223**	**-0.140**	**-0.053**	**-0.136**
	0.083	0.078	0.112	0.096	0.087	0.130	0.108	0.079	0.176
Age=20	**0.012**	**-0.165**		**-0.165**	**-0.335**		**-0.026**	**-0.039**	
	0.074	0.091		0.141	0.157		0.089	0.097	
Age=21	**0.054**	**-0.271**		**-0.053**	**-0.258**		**0.029**	**-0.110**	
	0.075	0.118		0.061	0.078		0.097	0.148	
Age=22	**-0.273**	**-0.542**		**-0.267**	**-0.379**		**-0.250**	**-0.295**	
	0.079	0.126		0.141	0.110		0.085	0.101	
Age=23	**-0.468**			**-0.386**			**-0.309**		
	0.097			0.101			0.109		
Age=24	**-0.410**			**-0.311**			**-0.330**		
	0.082			0.093			0.119		
R-square adj	0.79			0.79			0.76		
N	3768			3702			3595		

Coefficients in bold (standard errors below) are relative changes in age-specific arrests rates for birth cohorts in repeal relative to non-repeal states. The omitted category is the1968-70 birth cohort. All specifications include fixed effects for interactions of age and state as well as age and year of birth. Standard errors have been adjusted for intra-class correlation within state and year of birth cells. All regressions have been weighted by the state population.

Table 6

Difference-in-difference Estimates of Relative Changes in Fertility Rates Among Teens and All Women in Repeal and Non-repeal States, 1969-1973

	Change in Ln Fertility Rates in Repeal vs. Non-repeal states			
	1970--69	1971--69	1972--69	1973--69
Teens	0.004	-0.112	-0.136	-0.161
Relative to 73-69 Δ		0.692	0.845	1.000
All women	-0.006	-0.051	-.057	-.062
Relative to 73-69 Δ		0.823	0.909	1.000

The difference-in-differences are as follows: $(\text{Ln } FR_{jt,repeal} - \text{Ln } FR_{j69,repeal}) - (\text{Log } FR_{jt,\ non\text{-}repeal} - \text{Ln } FR_{j69,non\text{-}repeal})$ where FR_{jt} is the fertility rate of age group j in year t and Ln is the natural logarithm.

Table 7

Relative Changes in Arrest and Homicide Rates among Cohorts Exposed to Legalized Abortion between 1969 & 1971 as Compared to Cohorts Unexposed between 1968 & 1970 in Repeal and Non-repeal States

	Crime Years					
	Age group exposed vs. unexposed to legalized abortion					
	1987 & 89	1988 & 90	1989 & 91	1990 & 92	1991 & 93	1992 & 94
	18 vs. 19	19 vs. 20	20 vs. 21	21 vs. 22	22 vs. 23	23 vs. 24
Crime category	(1)	(2)	(3)	(4)	(5)	(6)
Ln Arrest rates						
Violent						
DD Repeal only	-.010	-.006	-.012	-.069	.022	-.003
	(.111)	(.127)	(.043)	(.071)	(.081)	(.155)
DDD	-.075	-.041	-.050	-.124	-.012	-.081
	(.124)	(.135)	(.062)	(.082)	(.106)	(.163)
Property						
DD Repeal only	.020	-.080	-.015	-.029	-.003	.015
	(.072)	(.073)	(.047)	(.059)	(.088)	(.045)
DDD	.004	-.083	-.048	-.093	-.014	-.062
	(.089)	(.085)	(.058)	(.069)	(.102)	(.067)
Murder						
DD Repeal only	.092	.210	.028	.091	-.032	-.019
	(.193)	(.187)	(.102)	(.169)	(.140)	(.202)
DDD	.053	.138	.039	-.030	.101	.053
	(.224)	(.221)	(.154)	(.202)	(.187)	(.237)
Ln Homicide rates						
Victimization-R						
DD Repeal only	.195	.053	-.211	.164	-.182	-.175
	(.148)	(.147)	(.135)	(.125)	(.117)	(.171)
DDD	.185	.031	-.344*	.104	-.055	-.218
	(.199)	(.191)	(.168)	(.152)	(.146)	(.197)
Victimization-B						
DD Repeal only	-.116	-.055	-.258	.162	-.220	.048
	(.207)	(.235)	(.178)	(.177)	(.169)	(.179)
DDD	-.211	-.065	-.439*	.122	-.061	.050
	(.249)	(.260)	(.201)	(.199)	(.203)	(.205)
Offending						
DD Repeal only	.001	.568	-.359*	-.079	.060	-.177
	(.171)	(.372)	(.174)	(.205)	(.097)	(.213)
DDD	-.097	.450	-.463*	-.127	.087	-.326
	(.214)	(.394)	(.211)	(.231)	(.145)	(.246)

Figures are difference-in-difference (DD) or difference in-difference-difference (DDD) estimates obtained from regressions of log arrest or log homicide rates on interactions of age, year of birth and repeal and non-repeal states.

Standard errors are in parentheses. All specifications include state fixed effects and standard errors have been adjusted for clustering within state and year of birth. In each column, cohorts from the younger age group went from unexposed to exposed to legalized abortion (in utero) between 1969 & 1971; the older age group in each column are from the 1968 or 1970 birth cohort and was unexposed to legalized abortion during this period. There are potentially 1224 observations given data by age, year of birth, and state. Due to missing data or zero homicides, actual sample sizes vary from 1146 for arrest rates for violent and property crime to 997 for murder arrest rates. Victimization-R is for homicide victimization by state of residence and Victimization-B is for homicide victimization by state of birth.
* p<.05

Table 8

Relative Changes in Homicide Rates for Teens (15-19) and Young Adults (20-24) in Repeal and Non-Repeal States by Exposure to Legalized Abortion, 1985-1997

	Ln Homicide Victimization		*Ln Homicide Offending*	
	Whites	**Blacks**	**Whites**	**Blacks**
Changes in Homicide (90-89)-(86-85):				
Teens newly exposed, young adults unexposed				
1. DD, teens-adults, repeal states	.294	.280	.561	.524
	(.123)	(.144)	(.140)	(.159)
2. DD, teens-adults, non-repeal states	.180	.407	.380	.432
	(.062)	(.074)	(.071)	(.082)
3. DDD (row 1- row 2)	.113	-.127	.181	.092
	(.137)	(.162)	(.157)	(.179)
R-squared	.851	.785	.801	.718
N	576	465	559	474
Changes in Homicide (97-96)-(91-90):				
Young adults newly exposed, teens already exposed				
4. DD, adults-teens, repeal states	-.113	.066	.002	.039
	(.123)	(.131)	(.150)	(.140)
5. DD, adults-teens, non-repeal states	-.314	-.057	-.226	.096
	(.061)	(.067)	(.078)	(.074)
6. DDD (row 4- row 5)	.200	.123	.228	-.058
	(.137)	(.148)	(.169)	(.158)
R-squared	.862	.688	.809	.637
N	774	657	735	642

Difference-in-difference-in-difference (DDD) estimates show relative changes in homicide rates between those exposed and unexposed to legalized abortion in repeal and non-repeal states [equation (2) in text]. Standard errors are in parentheses. Models include controls for prisoners, police, income, poverty, AFDC generosity, concealed gun laws and beer tax as in Donohue and Levitt (2001). All models include state and year fixed effects. Regressions are weighted by the race-specific population 15 to 24 years of age.

Table 9

Elasticity of Homicide Rates with Respect to Lagged Fertility Rate Among Teens (15-19) and Young Adults (20-24) in Repeal and Non-Repeal States by Exposure to Legalized Abortion, 1985-1997

	Ln Homicide Victimization		*Ln Homicide Offending*	
	Whites	**Blacks**	**Whites**	**Blacks**
Changes in Homicide (90-89)-(86-85):				
Teens newly exposed, young adults unexposed				
1. DD, teens-adults, repeal states	.054	-.054	.104	.056
	(.037)	(.046)	(.043)	(.051)
2. DD, teens-adults, non-repeal states	.043	-.001	.065	.054
	(.028)	(.036)	(.033)	(.041)
3. DDD (row 1- row 2)	.012	-.053	.038	.002
	(.032)	(.038)	(.038)	(.051)
R-squared	.854	.795	.802	.718
N	569	463	553	472
Changes in Homicide (97-96)-(91-90):				
Young adults newly exposed, teens already exposed				
4. DD, adults-teens, repeal states	.069	.078	.090	.078
	(.039)	(.044)	(.049)	(.047)
5. DD, adults-teens, non-repeal states	-.072	.067	-.038	.038
	(.021)	(.044)	(.027)	(.047)
6. DDD (row 4- row 5)	.141	-.011	.129	.039
	(.039)	(.024)	(.049)	(.027)
R-squared	.872	.690	.816	.637
N	772	657	733	641

Figures show the relative change in crime rates given a one percent change in lagged fertility rates between those exposed and unexposed to legalized abortion in repeal as compared with non-repeal states [equation (3) in text]. Standard errors are in parentheses. The fertility rate is lagged 18 years for teen homicide and 22 years for young adult homicide. Models include controls for prisoners, police, income, poverty, AFDC generosity, concealed gun laws and beer tax as in Donohue and Levitt (2001). All models include state and year fixed effects. Regressions are weighted by the race-specific population 15 to 24 years of age.

Table 10
Coefficients (standard errors) on the Effective Abortion Ratio in Index Crime Regressions

	Ln Violent Crime		*Ln Property Crime*		*Ln Murder*	
	(1)	(2)	(3)	(4)	(5)	(6)
			Full Sample 1985-1997			
Effective abortion ratio (x 100)	-.137	-.129	-.095	-.091	-.108	-.121
	(.023)	(.024)	(.018)	(.018)	(.036)	(.047)
Log effective abortion ratio	.138	.122	.147	.138	.057	.052
	(.036)	(.036)	(.028)	(.025)	(.028)	(.034)
Mean abortion (standard deviation)	77.1	(83.2)	132.3	(116.5)	51.0	(66.6)
Min -- Max	0.01	750.9	0.3	976.5	0.005	621.9
N	663	663	663	663	663	663
			Repeal States 1991-1997			
Effective abortion ratio (x 100)	-.238	-.087	-.103	-.088	-.446	-.221
	(.104)	(074)	(.115)	(.092)	(.187)	(.151)
Log effective abortion ratio	1.846	2.136	1.304	2.337	2.618	2.567
	(.308)	(674)	(.295)	(.997)	(.389)	(.953)
Mean abortion (standard deviation)	238.0	(87.0)	350.6	(99.3)	180.3	(78.9)
Min -- Max	45.8	750.9	85.7	976.5	27.1	621.9
N	42	42	42	42	42	42
			Non-repeal States 1991-1997			
Effective abortion ratio (x100)	-.141	-.161	-.122	-.146	-.200	-.227
	(.042)	(.041)	(.042)	(.022)	(.061)	(.050)
Log effective abortion ratio,	.218	.242	.133	.110	.024	.107
	(.076)	(.072)	(.062)	(.054)	(.079)	(.081)
Mean abortion (standard deviation)	88.8	(49.1)	151.6	(68.7)	58.8	(40.0)
Min -- Max	3.5	228.8	10.4	328.2	0.8	179.6
N	315	315	315	315	315	315
Model includes other covariates	No	Yes	No	Yes	No	Yes

The first row in the top panel replicates the results from Table 4 of Donohue and Levitt (2001). The second row shows estimates from a specification in which the natural logarithm of the effective abortion ratio is used instead of the level. The third row shows the weighted mean of the effective abortion ratio, its standard deviation (in parentheses), and the minimum and maximum abortion ratio for the sample. Results in the middle and bottom panel show the same estimates but for the designated sub-sample.

Figure 1
Fertility and Abortion Rates for Repeal (R) and Non-Repeal States (NR)

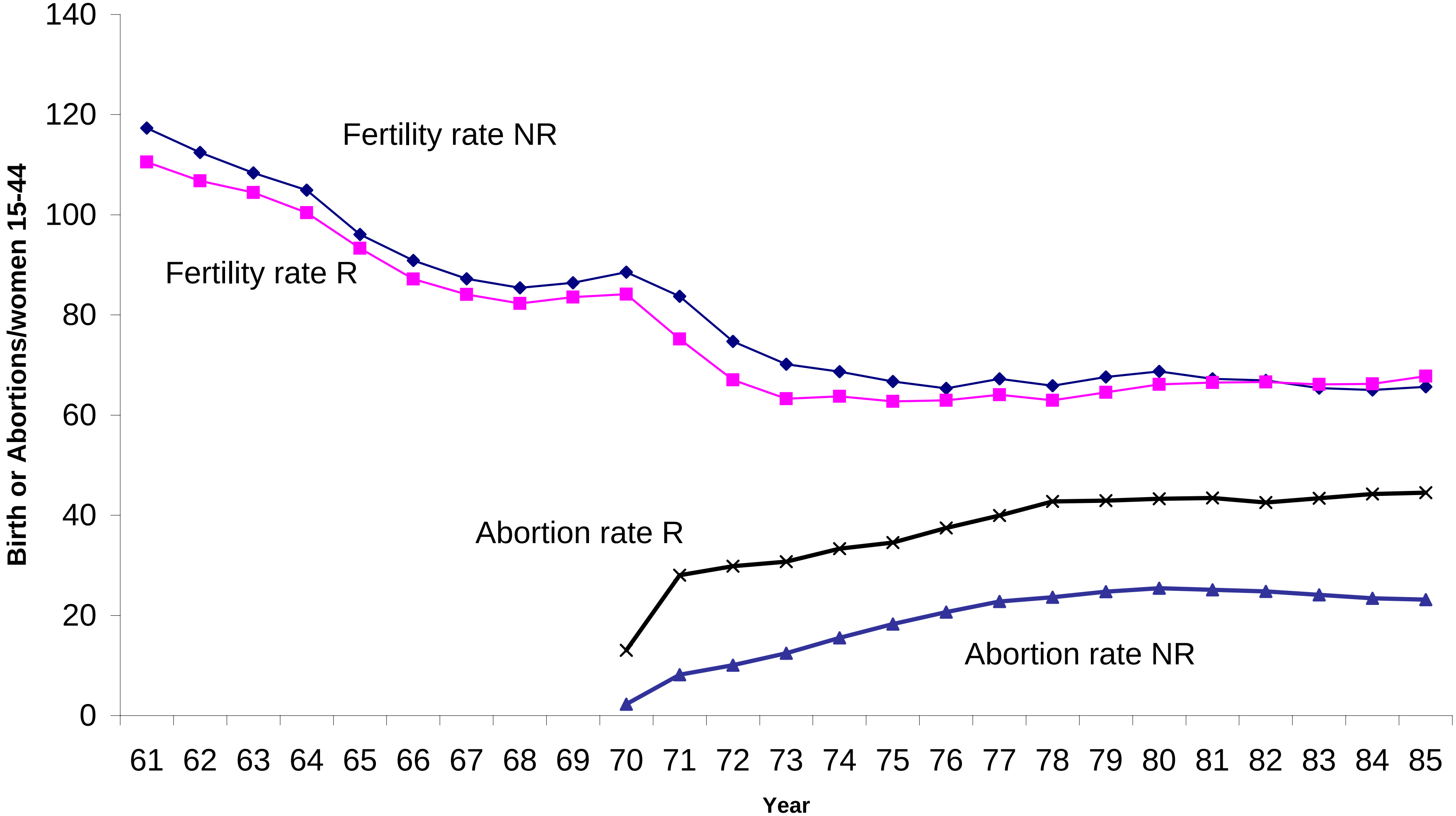


Figure 2a.
Ln Violent Crime Arrest Rates for 16 & 17 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)

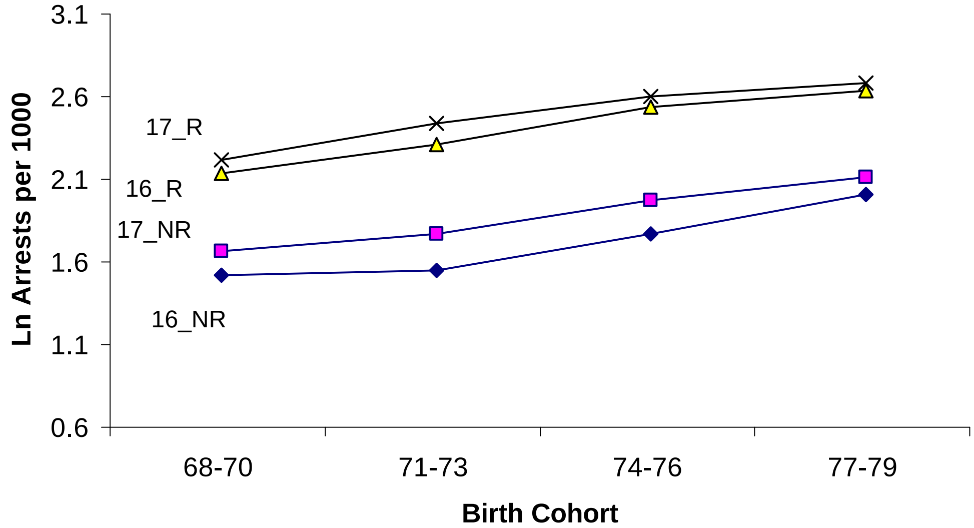
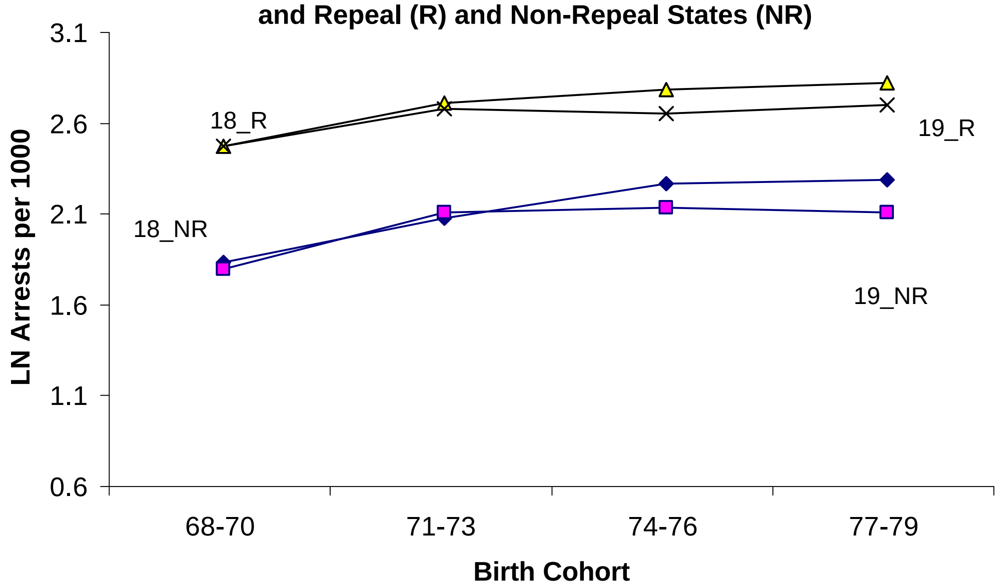


Figure 2b
Violent Crime Arrest Rates for 18 & 19 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)



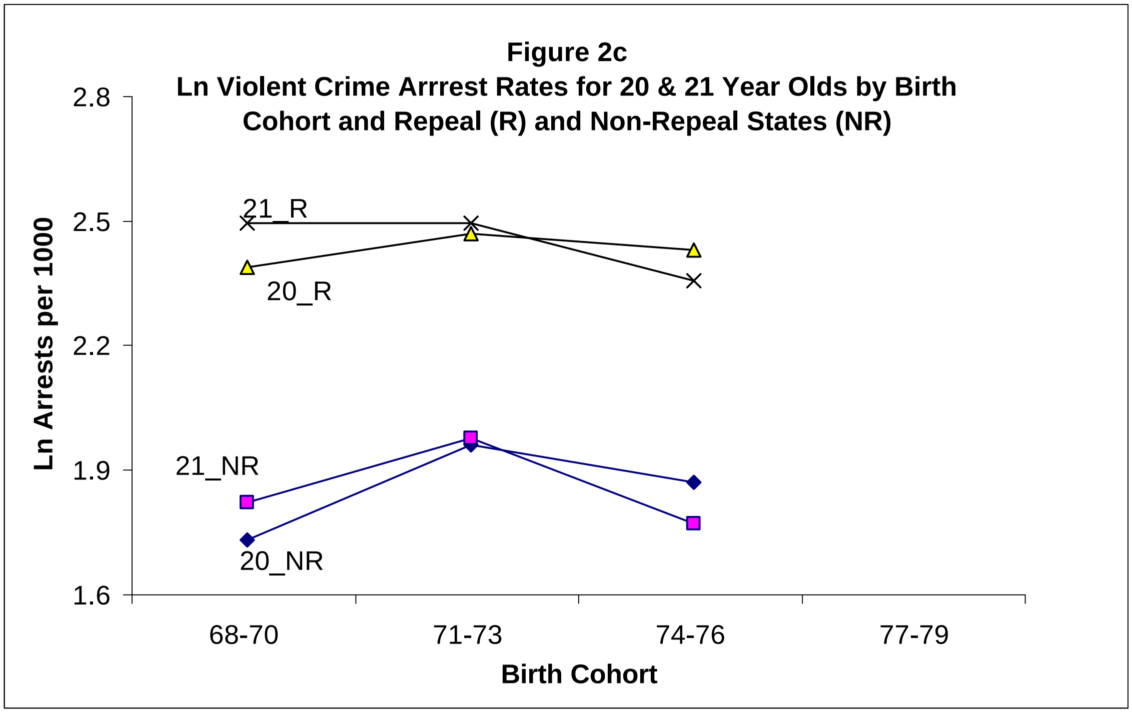


Figure 2c
Ln Violent Crime Arrrest Rates for 20 & 21 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)

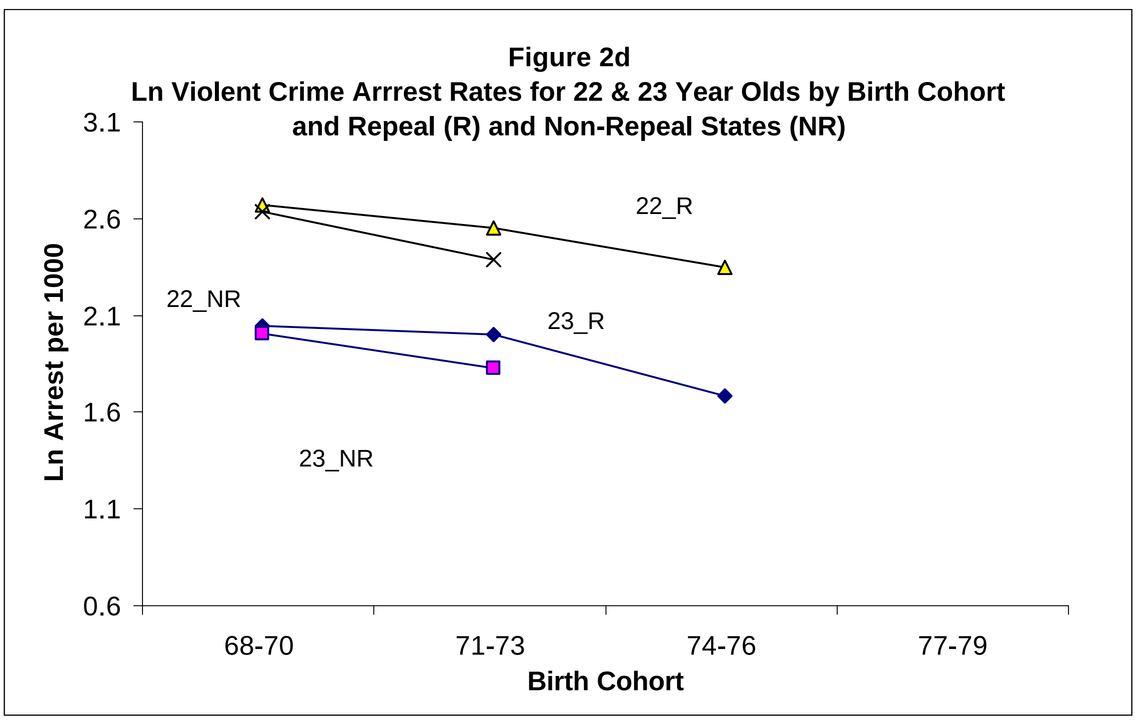


Figure 2d
Ln Violent Crime Arrrest Rates for 22 & 23 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)

Figure 3a
Ln Property Crime Arrrest Rates for 16 & 17 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)

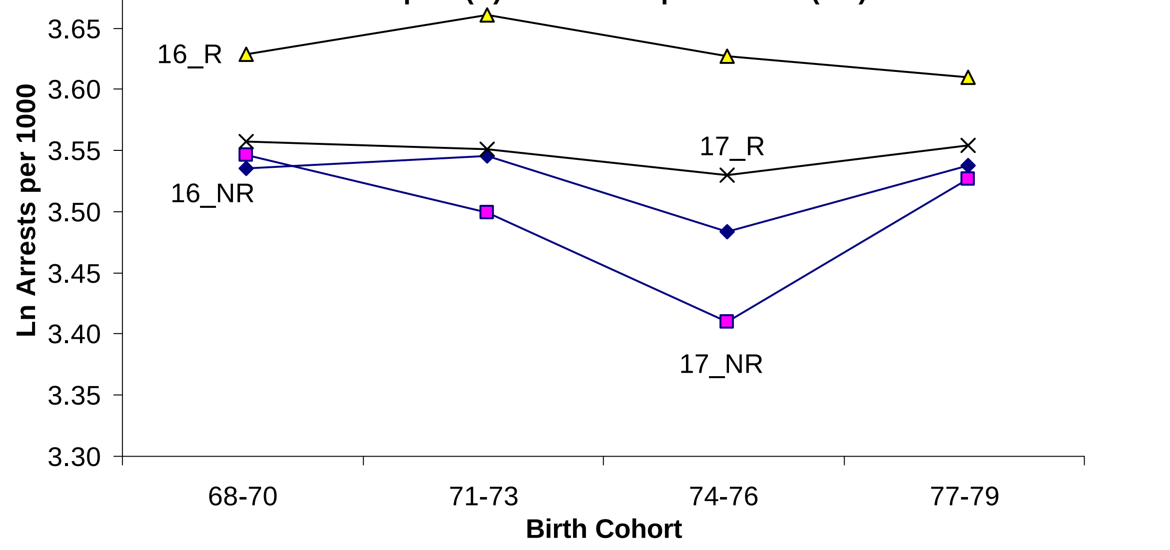
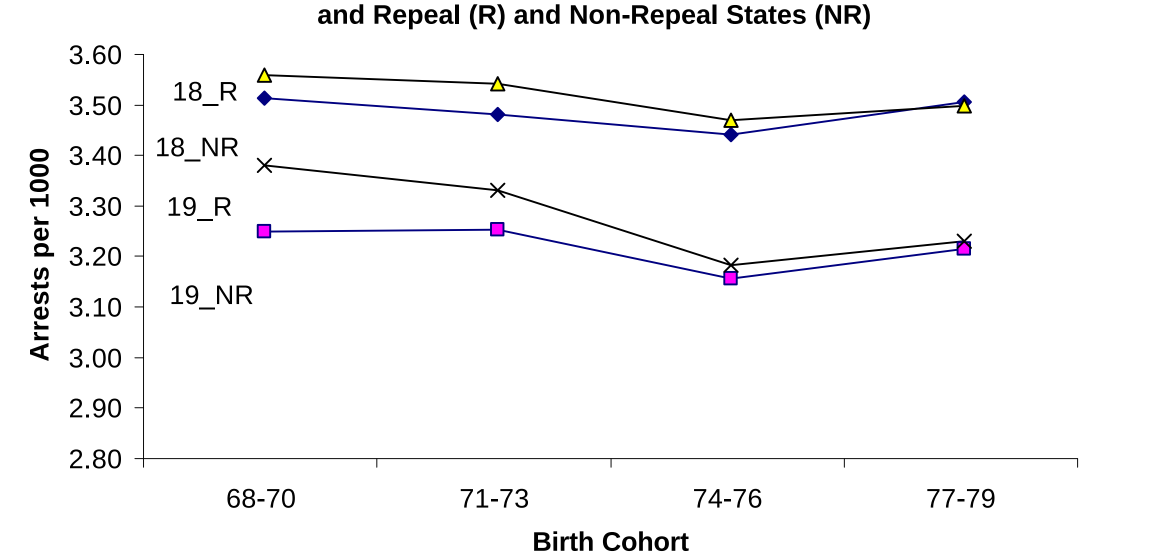


Figure 3b
Ln Property Crime Arrest Rates for 18 & 19 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)



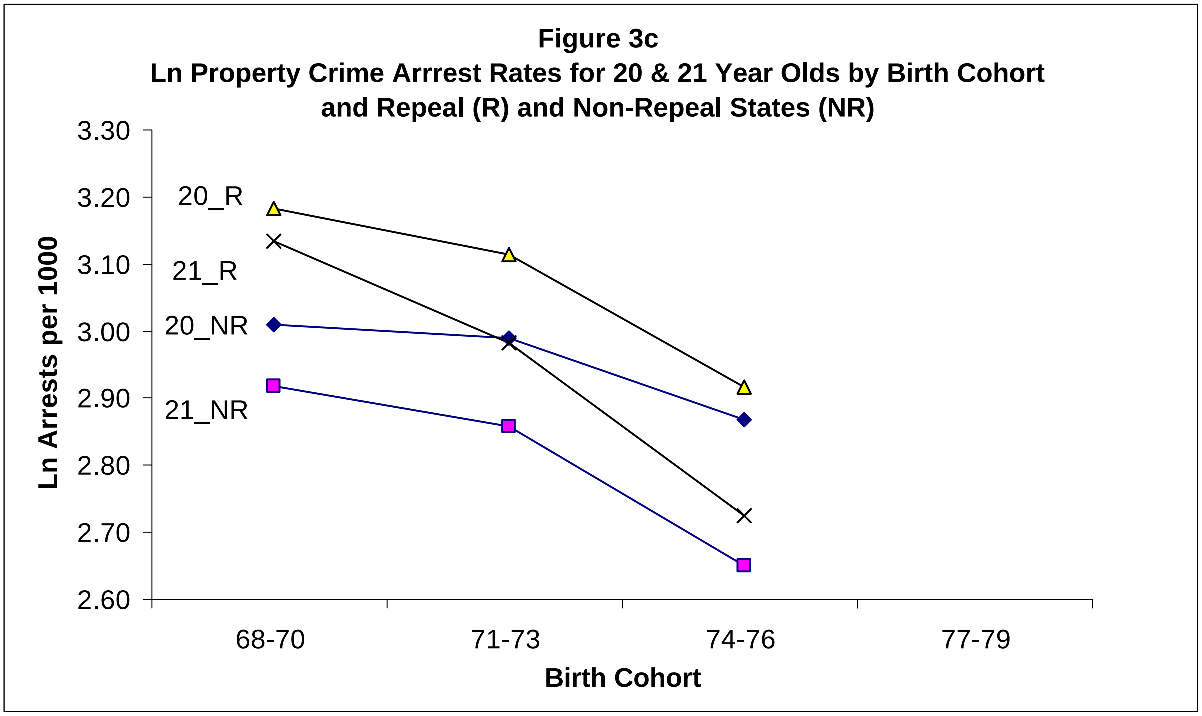


Figure 3c
Ln Property Crime Arrrest Rates for 20 & 21 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
3.30
3.20
3.10
3.00
2.90
2.80
2.70
2.60
Ln Arrests per 1000
20_R
21_R
20_NR
21_NR
68-70
71-73
74-76
77-79
Birth Cohort

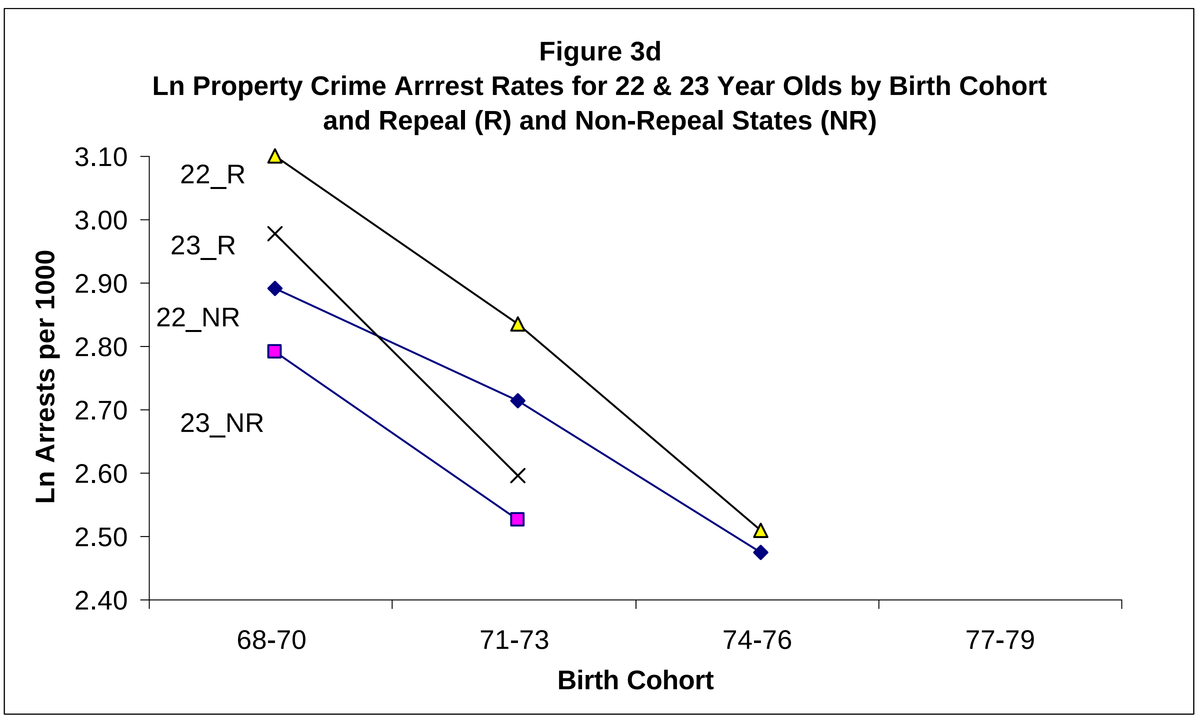


Figure 3d
Ln Property Crime Arrrest Rates for 22 & 23 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
3.10
3.00
2.90
2.80
2.70
2.60
2.50
2.40
Ln Arrests per 1000
22_R
23_R
22_NR
23_NR
68-70
71-73
74-76
77-79
Birth Cohort

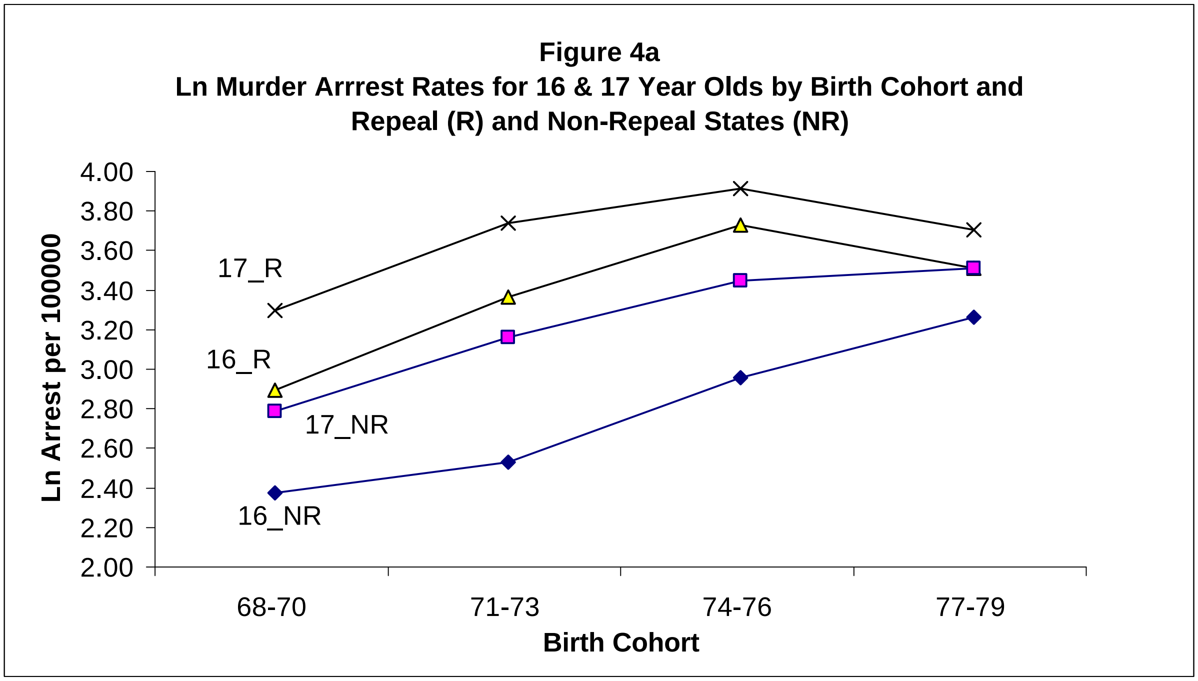


Figure 4a
Ln Murder Arrrest Rates for 16 & 17 Year Olds by Birth Cohort and
Repeal (R) and Non-Repeal States (NR)
Ln Arrest per 100000
4.00
3.80
3.60
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
17_R
16_R
17_NR
16_NR
68-70
71-73
74-76
77-79
Birth Cohort

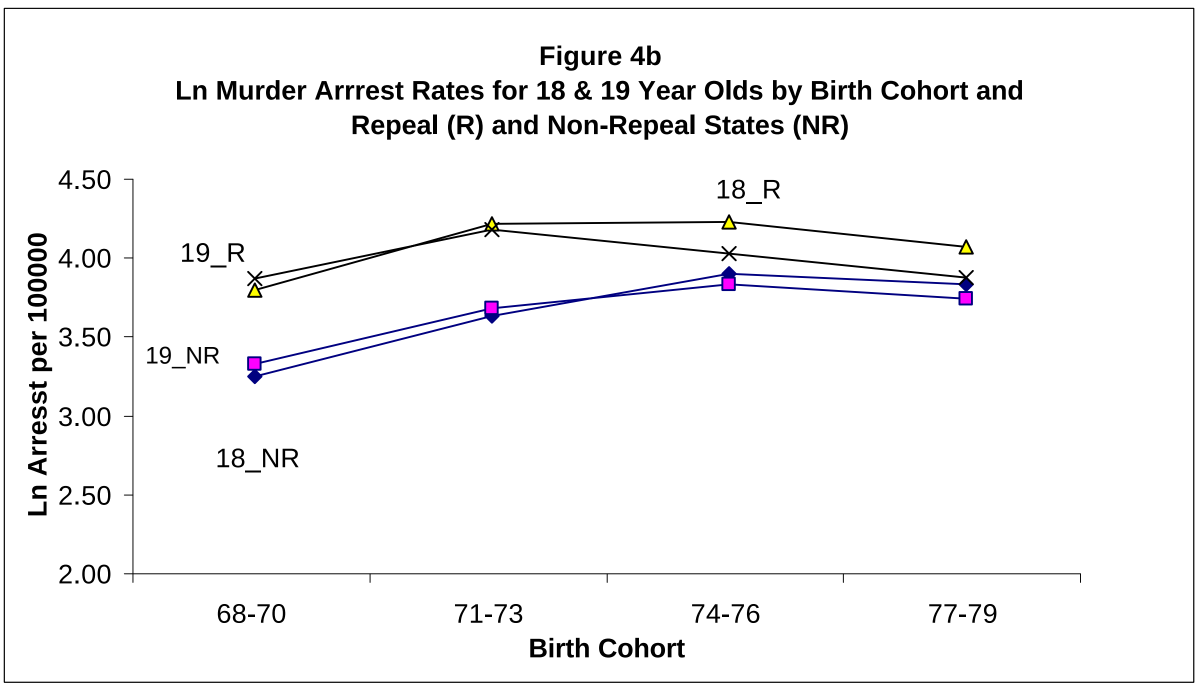


Figure 4b
Ln Murder Arrrest Rates for 18 & 19 Year Olds by Birth Cohort and
Repeal (R) and Non-Repeal States (NR)
Ln Arresst per 100000
4.50
4.00
3.50
3.00
2.50
2.00
18_R
19_R
19_NR
18_NR
68-70
71-73
74-76
77-79
Birth Cohort

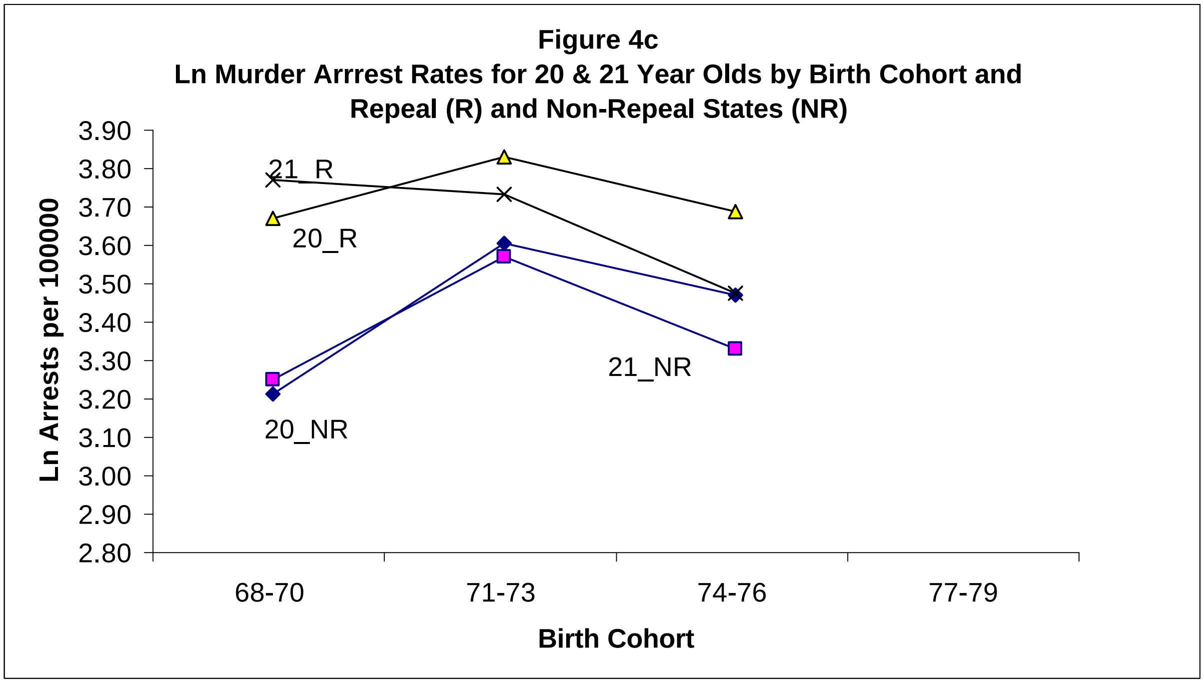


Figure 4c
Ln Murder Arrrest Rates for 20 & 21 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
3.90
3.80
3.70
3.60
3.50
3.40
3.30
3.20
3.10
3.00
2.90
2.80
21_R
20_R
21_NR
20_NR
68-70
71-73
74-76
77-79
Birth Cohort

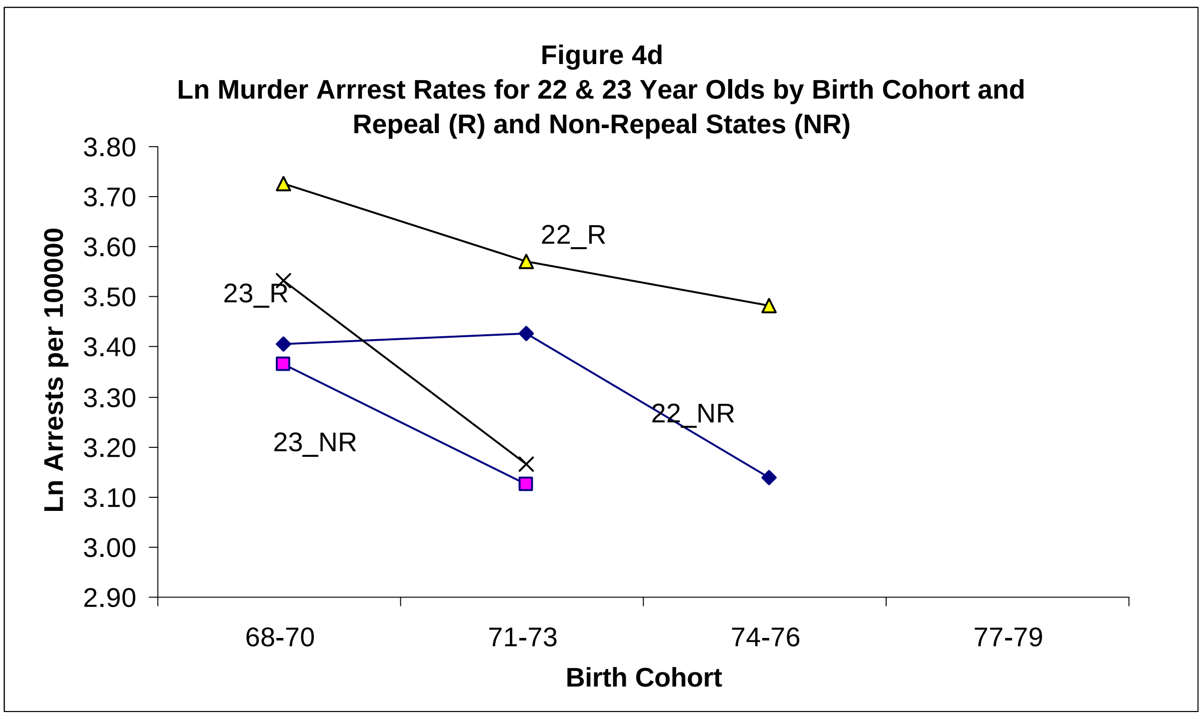


Figure 4d
Ln Murder Arrrest Rates for 22 & 23 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
3.80
3.70
3.60
3.50
3.40
3.30
3.20
3.10
3.00
2.90
22_R
23_R
22_NR
23_NR
68-70
71-73
74-76
77-79
Birth Cohort

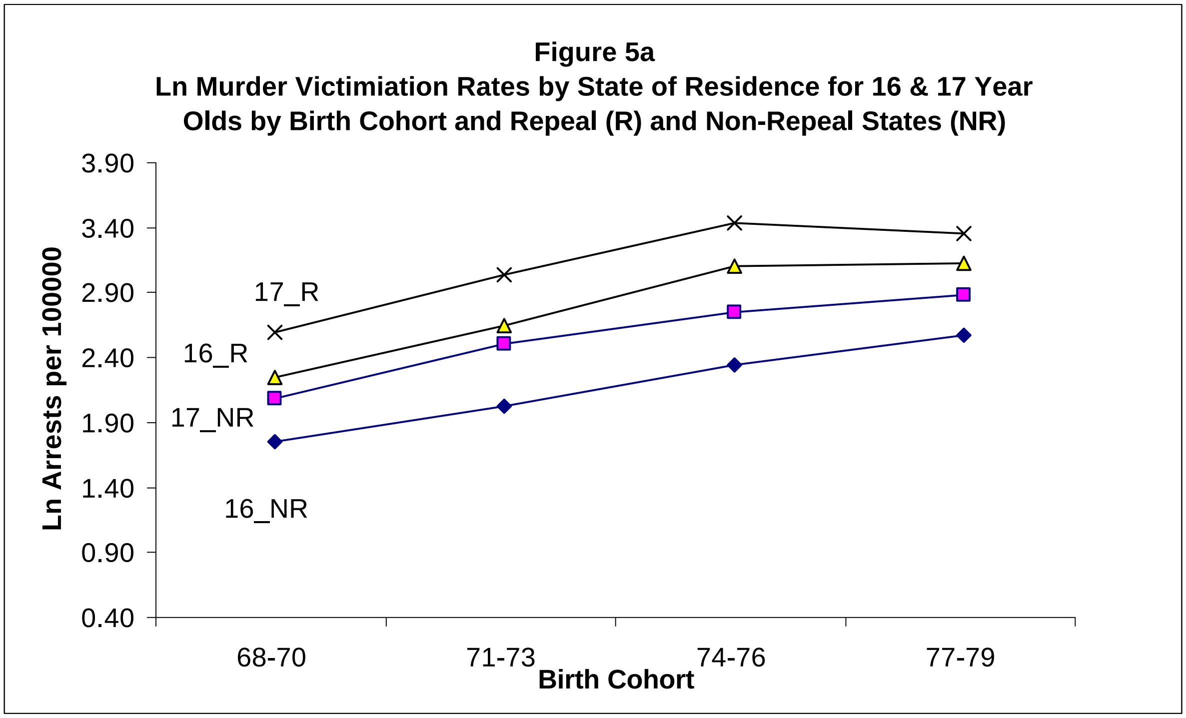


Figure 5a
Ln Murder Victimiation Rates by State of Residence for 16 & 17 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
3.90
3.40
2.90
2.40
1.90
1.40
0.90
0.40
17_R
16_R
17_NR
16_NR
68-70
71-73
74-76
77-79
Birth Cohort

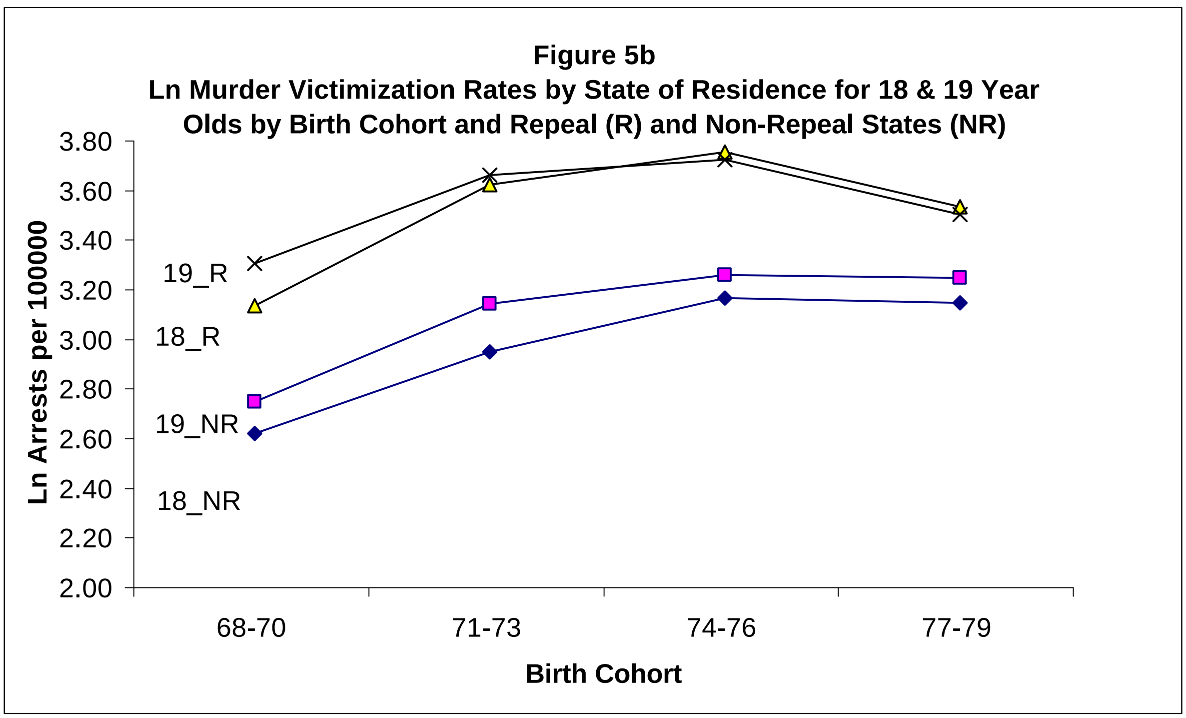
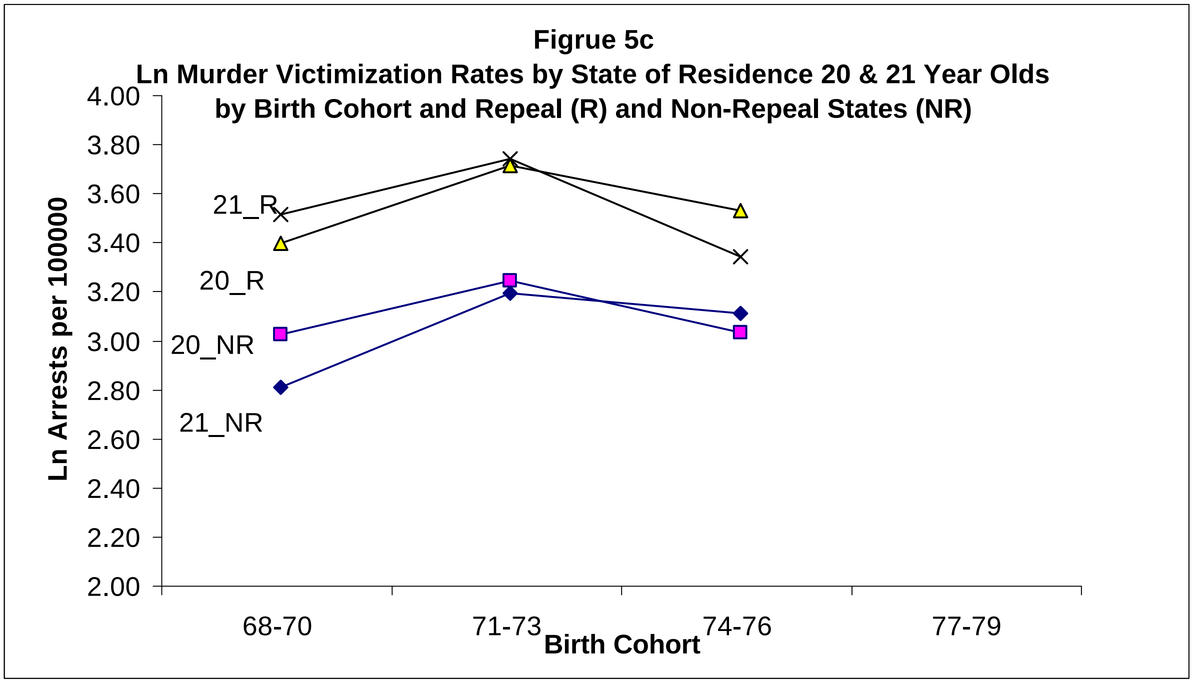


Figure 5b
Ln Murder Victimization Rates by State of Residence for 18 & 19 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
3.80
3.60
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
19_R
18_R
19_NR
18_NR
68-70
71-73
74-76
77-79
Birth Cohort


Figrue 5c
Ln Murder Victimization Rates by State of Residence 20 & 21 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
4.00
3.80
3.60
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
21_R
20_R
20_NR
21_NR
68-70
71-73
74-76
77-79
Birth Cohort

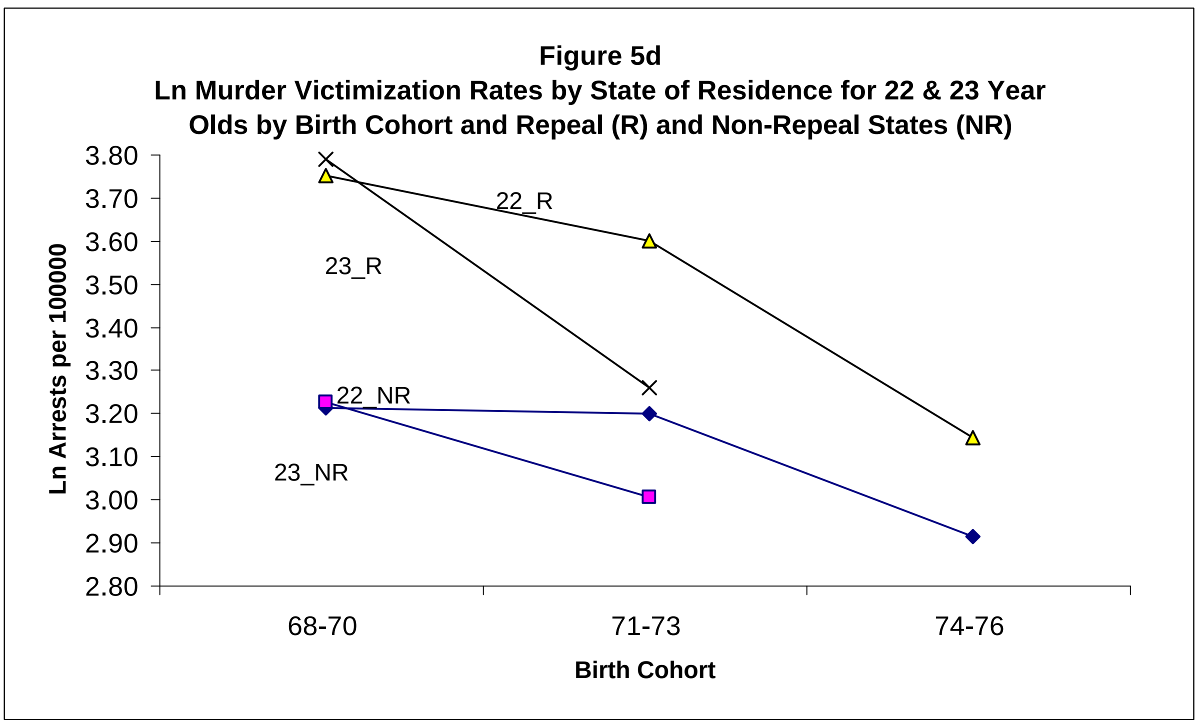


Figure 5d
Ln Murder Victimization Rates by State of Residence for 22 & 23 Year Olds by Birth Cohort and Repeal (R) and Non-Repeal States (NR)
Ln Arrests per 100000
3.80
3.70
3.60
3.50
3.40
3.30
3.20
3.10
3.00
2.90
2.80
22_R
23_R
22_NR
23_NR
68-70
71-73
74-76
Birth Cohort

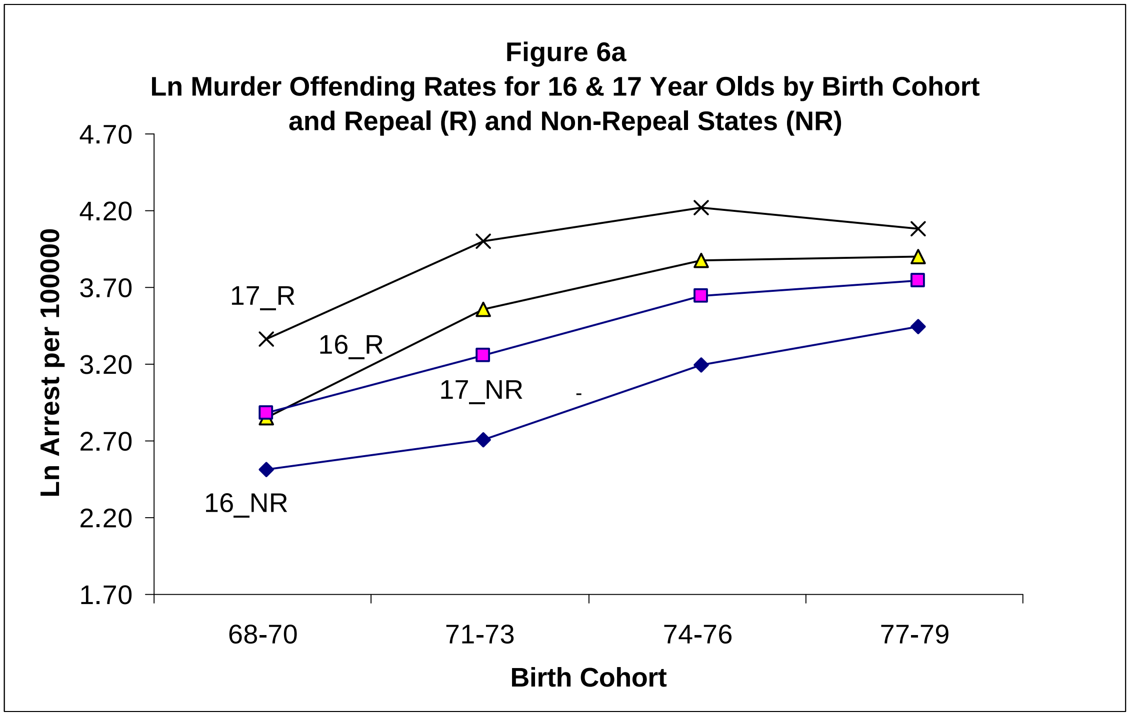


Figure 6a
Ln Murder Offending Rates for 16 & 17 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
Ln Arrest per 100000
4.70
4.20
3.70
3.20
2.70
2.20
1.70
17_R
16_R
17_NR
16_NR
68-70
71-73
74-76
77-79
Birth Cohort

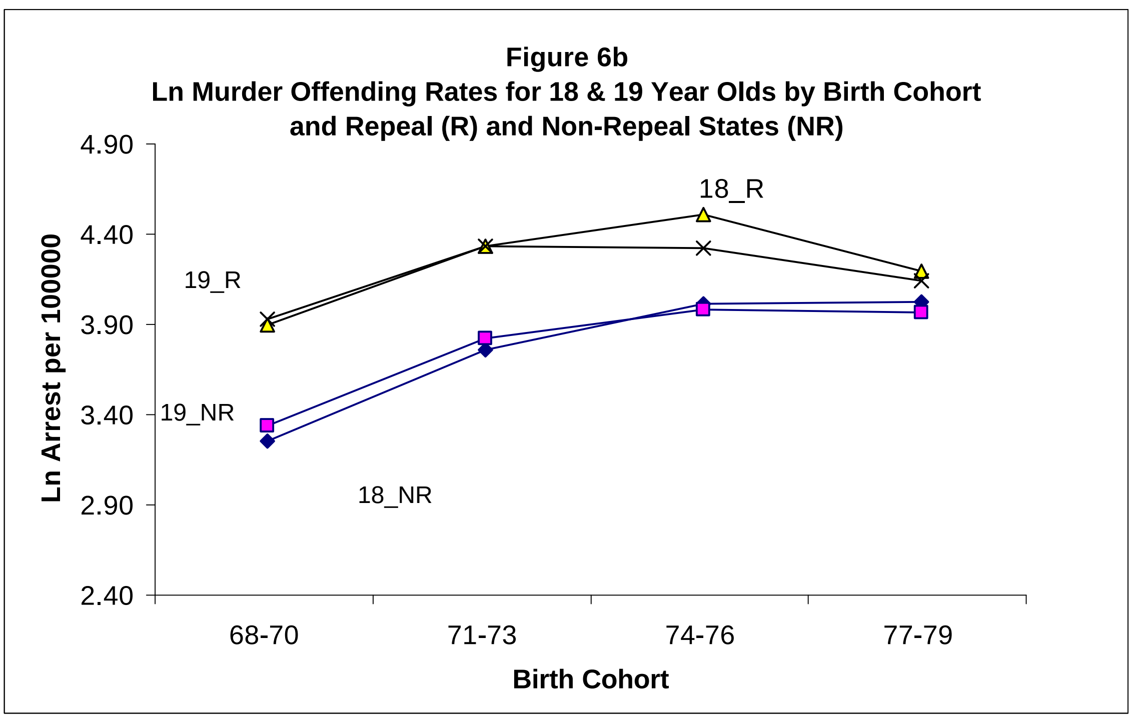


Figure 6b
Ln Murder Offending Rates for 18 & 19 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
Ln Arrest per 100000
4.90
4.40
3.90
3.40
2.90
2.40
18_R
19_R
19_NR
18_NR
68-70
71-73
74-76
77-79
Birth Cohort

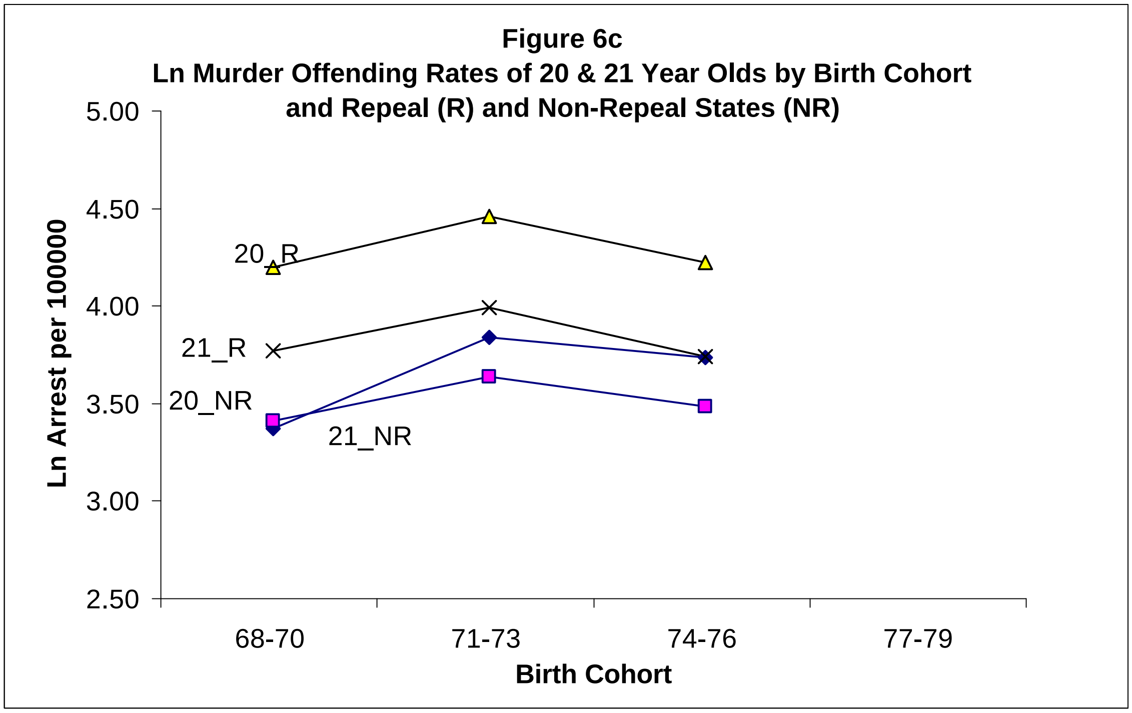


Figure 6c
Ln Murder Offending Rates of 20 & 21 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
Ln Arrest per 100000
5.00
4.50
4.00
3.50
3.00
2.50
20_R
21_R
20_NR
21_NR
68-70
71-73
74-76
77-79
Birth Cohort

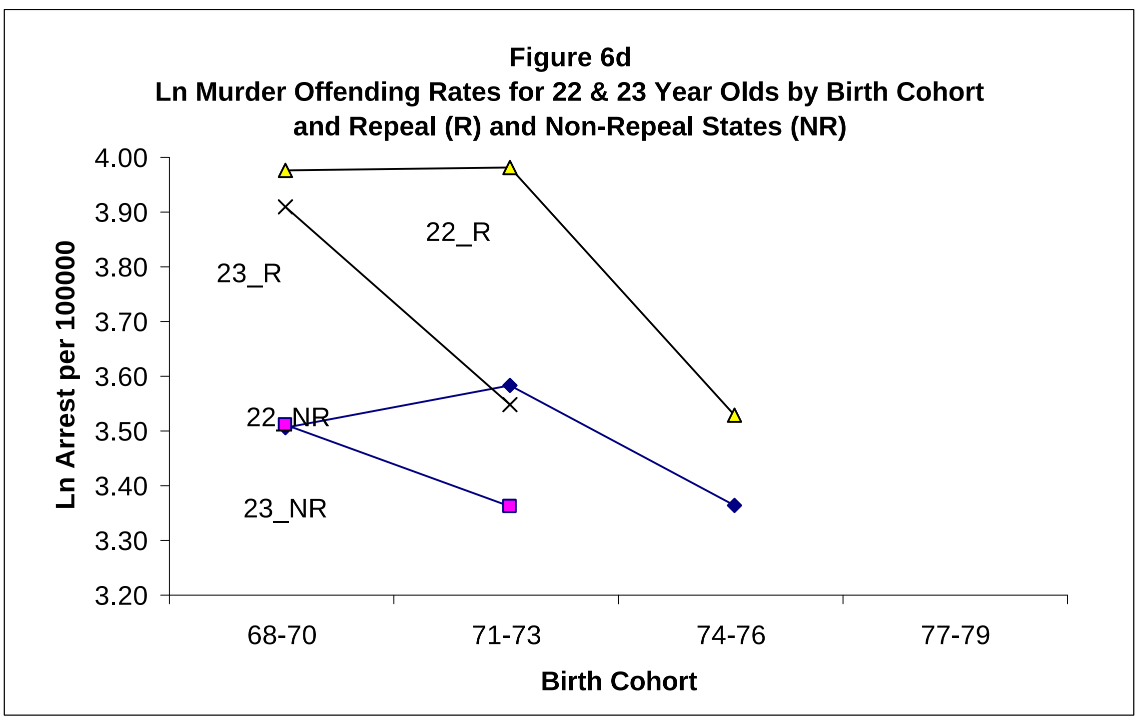


Figure 6d
Ln Murder Offending Rates for 22 & 23 Year Olds by Birth Cohort
and Repeal (R) and Non-Repeal States (NR)
Ln Arrest per 100000
4.00
3.90
3.80
3.70
3.60
3.50
3.40
3.30
3.20
22_R
23_R
22_NR
23_NR
68-70
71-73
74-76
77-79
Birth Cohort

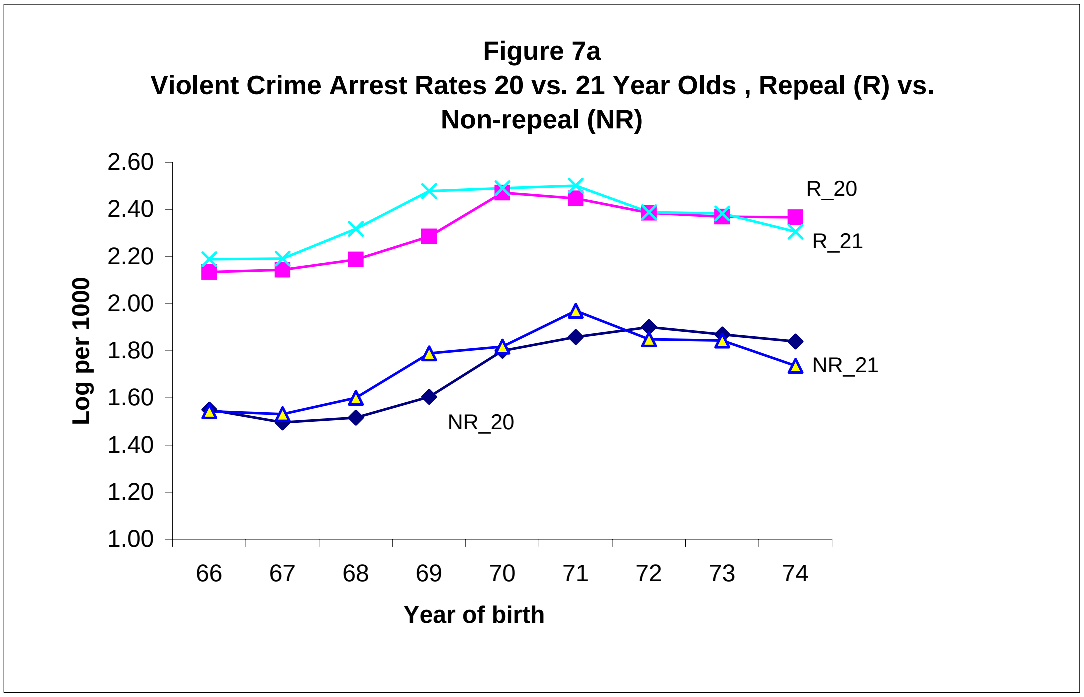


Figure 7a
Violent Crime Arrest Rates 20 vs. 21 Year Olds , Repeal (R) vs. Non-repeal (NR)
Log per 1000
2.60
2.40
2.20
2.00
1.80
1.60
1.40
1.20
1.00
66
67
68
69
70
71
72
73
74
Year of birth
R_20
R_21
NR_21
NR_20

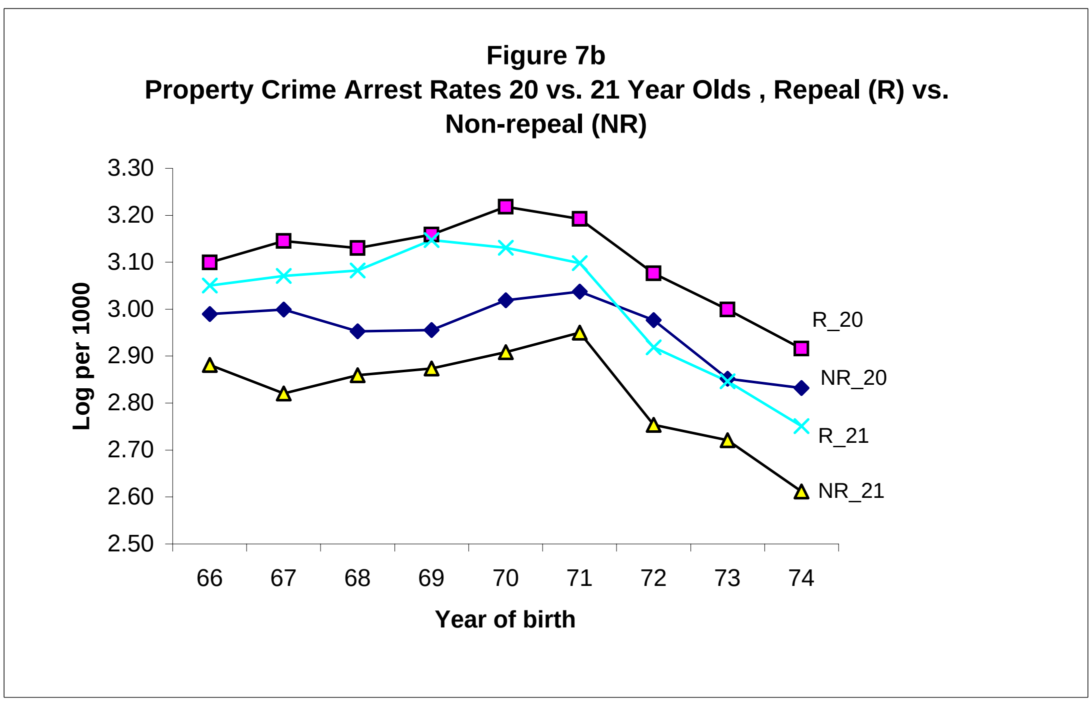


Figure 7b
Property Crime Arrest Rates 20 vs. 21 Year Olds , Repeal (R) vs. Non-repeal (NR)
Log per 1000
3.30
3.20
3.10
3.00
2.90
2.80
2.70
2.60
2.50
66
67
68
69
70
71
72
73
74
Year of birth
R_20
NR_20
R_21
NR_21

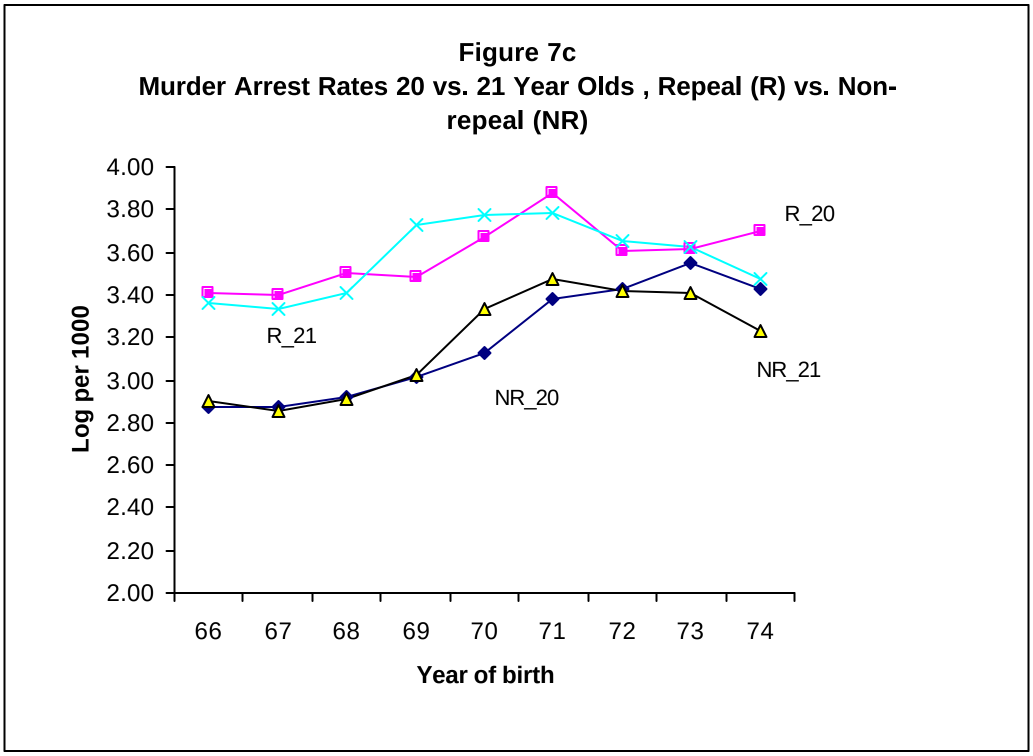


Figure 7c
Murder Arrest Rates 20 vs. 21 Year Olds , Repeal (R) vs. Non-repeal (NR)
Log per 1000
4.00
3.80
3.60
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
R_20
R_21
NR_21
NR_20
66
67
68
69
70
71
72
73
74
Year of birth

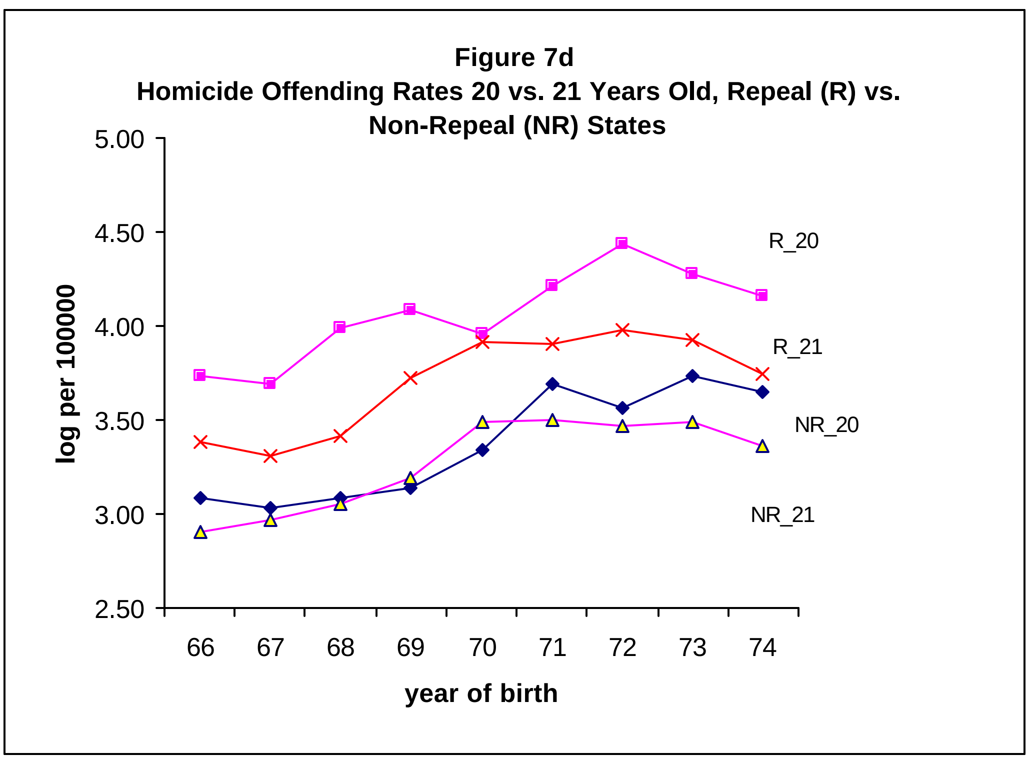


Figure 7d
Homicide Offending Rates 20 vs. 21 Years Old, Repeal (R) vs. Non-Repeal (NR) States
log per 100000
5.00
4.50
4.00
3.50
3.00
2.50
R_20
R_21
NR_20
NR_21
66
67
68
69
70
71
72
73
74
year of birth

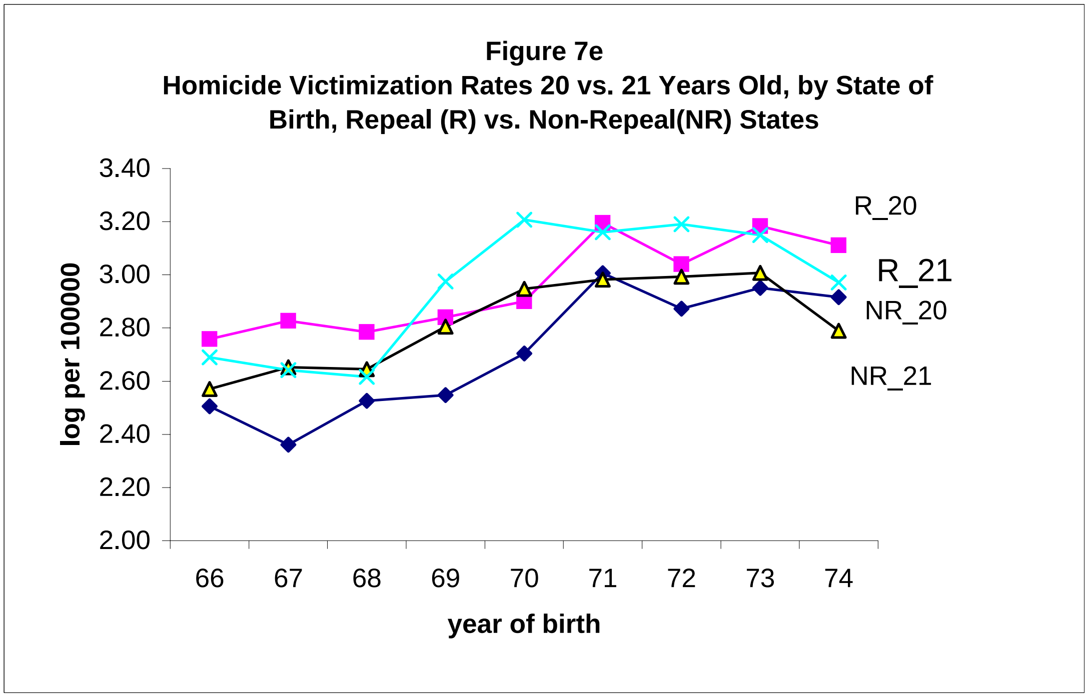


Figure 7e
Homicide Victimization Rates 20 vs. 21 Years Old, by State of Birth, Repeal (R) vs. Non-Repeal(NR) States
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
log per 100000
66
67
68
69
70
71
72
73
74
year of birth
R_20
R_21
NR_20
NR_21

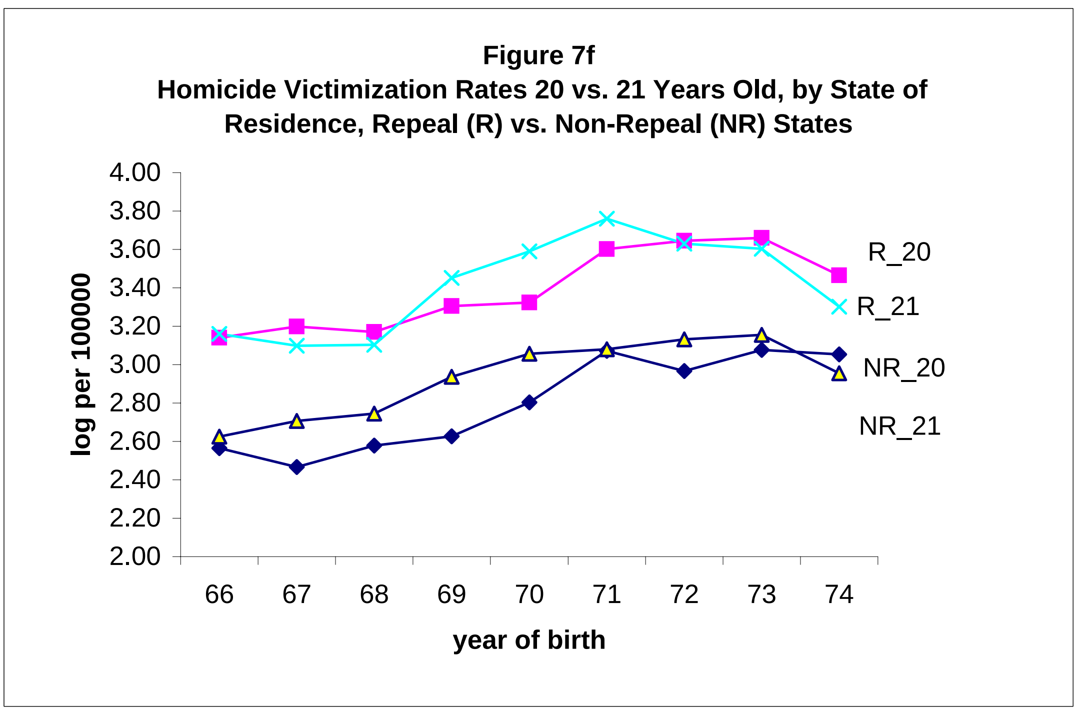


Figure 7f
Homicide Victimization Rates 20 vs. 21 Years Old, by State of Residence, Repeal (R) vs. Non-Repeal (NR) States
4.00
3.80
3.60
3.40
3.20
3.00
2.80
2.60
2.40
2.20
2.00
log per 100000
66
67
68
69
70
71
72
73
74
year of birth
R_20
R_21
NR_20
NR_21

Figure 8
White Homicide Victimization Rates for Teens (15-19) and Young Adults (20-24) by Repeal (R) and Non-repeal(NR) States 1985-1997

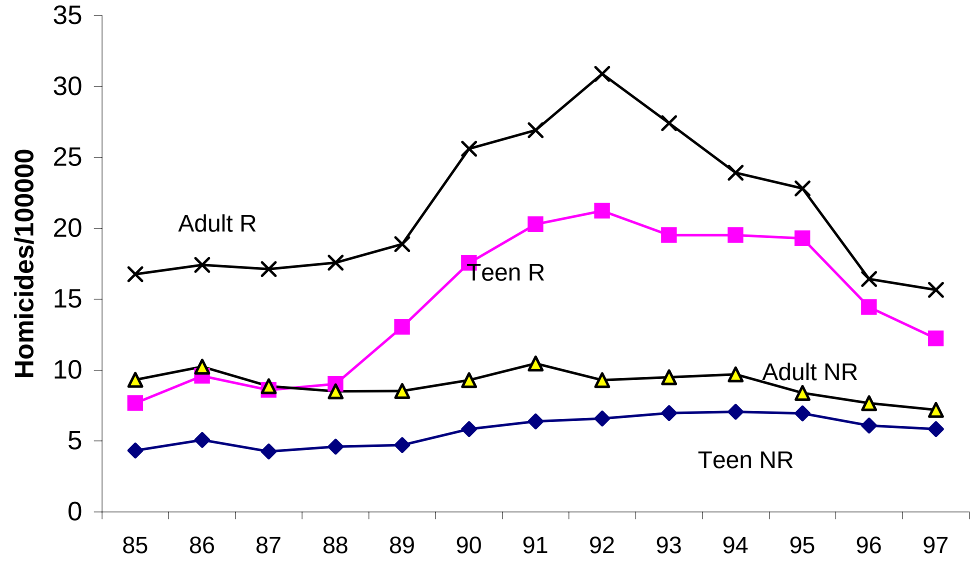


Figure 9
Black Homicide Victimization Rates for Teens (15-19) and Young Adults (20-24) by Repeal (R) and Non-repeal(NR) States 1985-1997

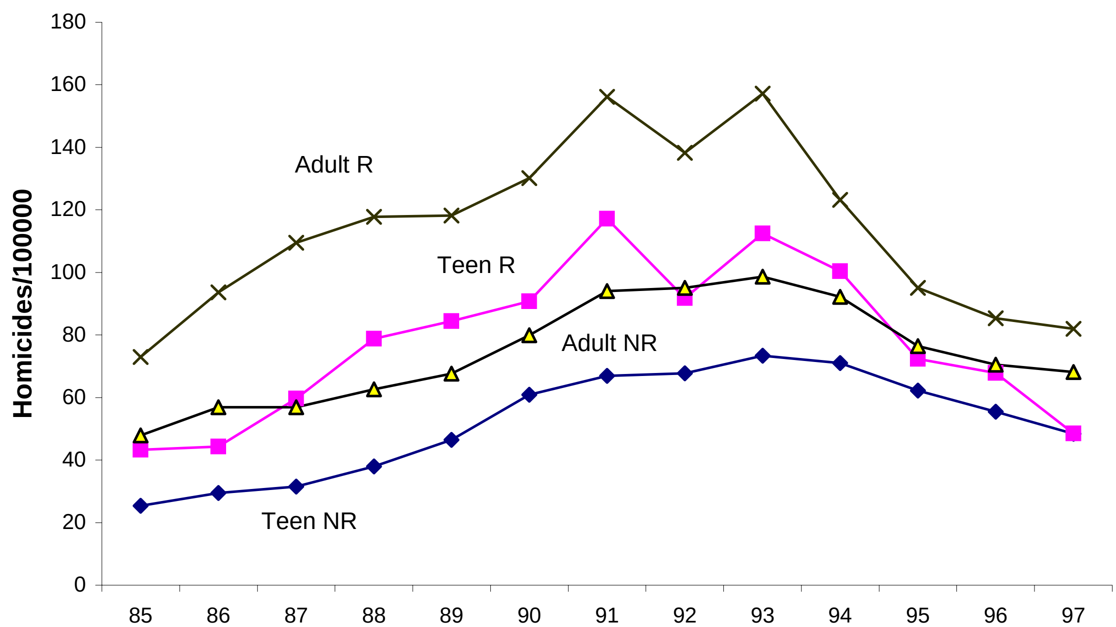


Figure 10
White Homicide Offending Rates for Teens (15-19) and Young Adults (20-24) by Repeal (R) and Non-repeal(NR) States 1985-1997

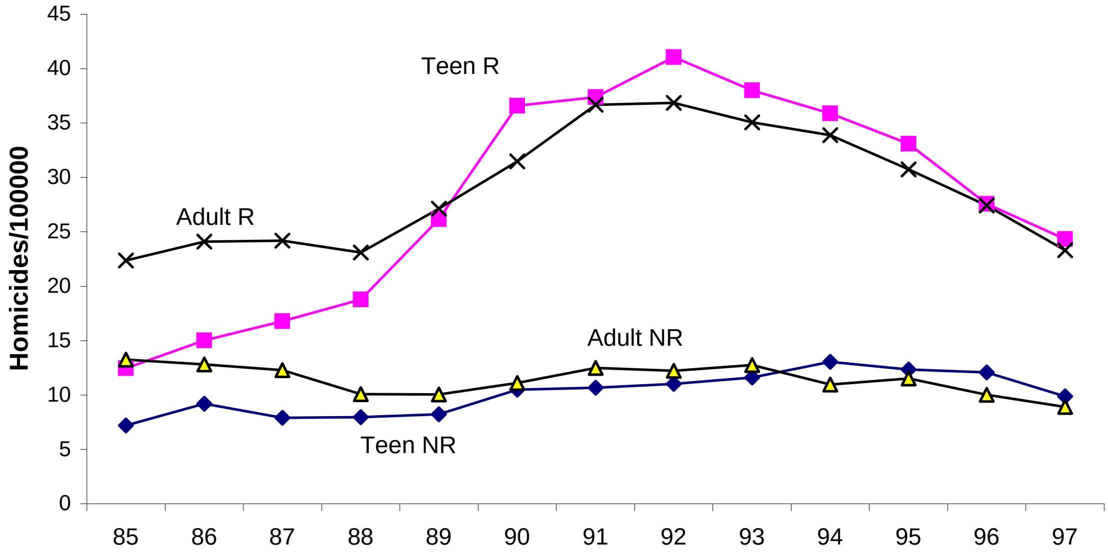


Figure 11
Black Homicide Offending Rates for Teens (15-19) and Young Adults (20-24) by Repeal (R) and Non-repeal(NR) States 1985-1997